$7.00 per volume
Single numbers $4.00

QUARTERLY

Two volumes per year

July, 1934
Volume XI, No. 1

Founded in 1927 by Edward Bradford Titchener and Carl Murchison

THE JOURNAL OF GENERAL PSYCHOLOGY

Experimental, Theoretical, Clinical, and Historical Psychology

JULY, 1934

Chaque article est suivi d'un résumé en français
Jedem Artikel wird ein Referat auf deutsch folgen

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AN INVESTIGATION OF THE CONDITIONED IRIDIC REFLEX*[1]

From the Department of Psychology of Ohio State University

LYNDE C. STECKLE AND SAMUEL RENSHAW

I. INTRODUCTION

In 1922 Cason (1) reported an experiment in which the pupillary reflex was conditioned to the auditory stimulation of a bell. Thirteen subjects were used. In each case conditioned dilation and contraction effects were obtained after approximately 400 trials.

Recently Hudgins (4), using a modification of Cason's technique, found iridic conditioning to the sound of a bell possible in 200-225 trials. He was further able to initiate conditioned responses by verbal commands. Conditioning was obtained when the commands were given by the experimenter and also when they were given by the subject himself. These responses were still found to be present after elimination of the auditory components involved. That is, they could be initiated by the vocal motor reactions alone.

The responses to the verbal commands were not subject to experimental extinction. Repeated tests served only to enhance them. While such results are not unique in the history of conditioning,[2] they are extremely infrequent. The absence of extinction is particularly unexpected with the so-called non-voluntary reflexes.

Since this finding is of major importance to the conditioned response doctrine, it seemed worth while to repeat the experiment.

II. PROBLEM

The problem involved in this investigation lies in an attempt to answer the following question: May the pupillary reflex become, through the process of conditioning, what is commonly called a voluntary response? That is, may a pupillary movement, e.g., contraction, after a period of training be elicited by verbal stimuli initiated by the subject himself?

*Accepted for publication by Carl Murchison of the Editorial Board.

[1]Our thanks are due to Mr. Horace Champney for valuable assistance throughout the work of these experiments.

[2]E.g., Israelson's experiment reported by G. Razran (6, pp. 112-113).

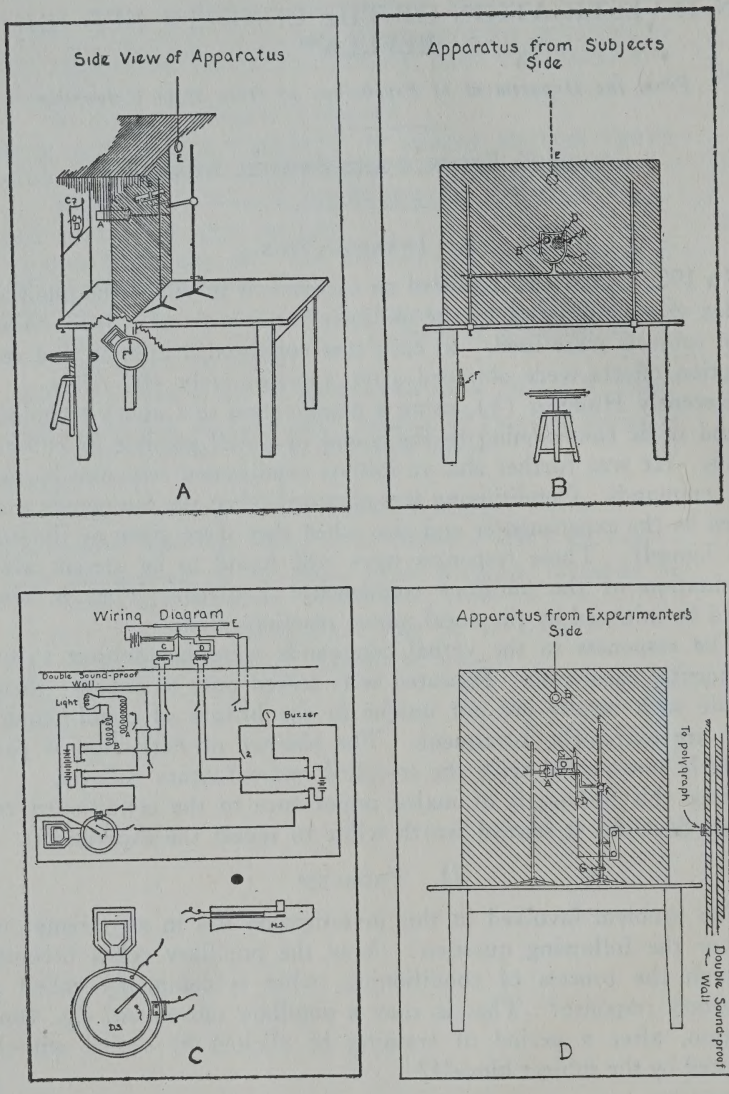

FIGURE 1

APPARATUS DETAILS

A-B

A—Light tube
B—Telescope
C—Head rest
D—Auxiliary lens
E—Buzzer
F—Dynamometer switch
Shaded portions indicate shielding screens.

D

A—Light tube
B—Buzzer
C—Telescope ocular
D—Extension lever
E—Zimmerman universal stand
F—Screw regulating horizontal movement.
Shaded portion indicates shielding screen.

C

A—Test light rheostat
B—Training light rheostat
C—Light circuit relay
D—Buzzer circuit relay
E—Polygraph panel board
F—Dynamometer switch
DS—Detail of dynamometer switch.
The indicator was insulated from the attached switch. The contact to the right of the indicator was insulated from the dial.
MS—Detail of switches 1 and 2.
These switches were strapped to the experimenter's left hand. Since they were movable, they could be plugged into any desired circuit.
All other switches were on the table in front of the experimenter.

Suppose a conditioned pupillary response is established to a complex stimulus situation, which includes vocalization on the part of the subject. Suppose further that this complex stimulus is progressively reduced so that only the subject's vocalization remains. If the conditioned pupillary response is now present, it might well be included in that class of behavior called voluntary. Furthermore, if this auto-verbally aroused conditioned response is not subject to experimental extinction, a tentative description of the genesis of such behavior might be made. A response, originally non-voluntary, has become subject to voluntary control.

Since this is essentially what Hudgins (4) reports, the need of a checking experiment is obvious. Such results, if valid, would be of great importance to theoretical and therapeutic psychology.

III. Apparatus

The apparatus was the same in principle as that used by Hudgins (4). Both consisted of an enclosed light source, an arrangement for delivering the light into the eye of the observer, a telescope with an ocular containing two parallel hairs, one being movable, and a recording device for measuring the magnitude of pupillary changes. (Figure 1, A.)

In our apparatus the light source was a 32-c.p. automobile headlight bulb set before a reflector in one end of a cardboard tube, 11

cm. long, 4 cm. in diameter, whitened on the inside. Over the other end of the tube was a circular piece of milk glass on which were mounted fine black cross hairs. These served as a fixation point. It was necessary to have the cross hairs fairly close to the eye (about 6 inches) so that a brightness comparable to that used by Hudgins (4) could be obtained. To avoid eye strain and *partially* to control the factors of accommodation and convergence, an auxiliary spherical-convex 3-diopter lens was introduced between the eye and the cross hairs.[3] With this lens in position the cross hairs were always brought sharply into focus whenever the light was on. (Figure 1, B.)

Two light intensities were used. The training light was the maximum intensity emitted by a 32-c.p. automobile headlight lamp operating on 7.5 volts of D.C. The test light had the same source but in this case the lamp was operated on 4 volts. (See Figure 1, C.) Photometric measurements gave intensities at the eye position as follows: training light, 37.66 meter candles; test light, 4.5 meter candles. The training light was used in all conditioning procedures; the test light was used in all tests. The intensity of the test light used here was 1 meter candle greater than that used by Hudgins (4). At an intensity of 3.5 meter candles with this apparatus, the pupil was not visible enough for accurate measurement.

Observations of the pupil were made by means of a telescope fitted with an ocular in which were two parallel hairs. One of these hairs was movable, the other was fixed. It was decided to measure the pupillary radius instead of the diameter since preliminary experiments indicated that the latter method was susceptible to errors of apparent visual movement. That is, when the movable cross hair was slowly drawn across the boundary of the pupil, the pupil would apparently *follow* the moving hair. It was hoped that such error could be partially controlled by "dividing" the pupil and using one half as a check on the other.

The movable hair was operated by an extension lever. (Figure 1, D.) The stationary hair could be kept in the center of the pupil by turning the thumb screw at the base of the Zimmerman universal stand on which the telescope was mounted. (Figure 1, D.)

---

[3]This principle was adapted from apparatus described by Howells and Cutler (3).

By means of this screw the telescope could be moved in the horizontal plane. Any other necessary adjustment could be accomplished readily because of the great flexibility of this mounting.

Although Hudgins (4) measured pupillary change by turning the screw which controlled the movable hair, in this experiment a lever was used for this purpose. (Figure 1, D.) This method was found to make for more rapid, and possibly more accurate, movement. After a little practice, pupillary changes could be readily followed. It was possible to obtain records of the light reflex, which was too rapid to be followed by Hudgins' method.

The sound of a buzzer, hand contractions and relaxations, and verbal commands were used as conditioning stimuli. The buzzer was an Edwards No. 750 watch-case type operated by a 6-volt storage battery. It gave a tone of 182 d.v. It was suspended in front of and slightly above the subject, but hidden behind the screen.

Hand contractions and relaxations were made by means of a Smedley dynamometer. Contact points attached to the dial were made to close two circuits simultaneously when touched by the indicator. A tension of 5 kgs. was used for all subjects. To avoid cutaneous irritation, the handles were well padded. The light and buzzer circuits were so arranged that they could be closed by the dynamometer or by separate switches near the experimenter. Either circuit could also be broken, although contact was being made by the dynamometer (Figure 1, C.)

A polygraph was used for recording. It was located in an adjoining room separated from the experimental room by a double wall. It could not be heard when in operation. Signal magnets recorded the incidence of all stimuli. Pupillary changes, amplified three times, were recorded by means of a braided silk thread fastened to the lower end of the ocular lever. This thread passed through a glass tube, inserted through the wall, to a sliding marker on the polygraph.

Verbal stimuli were recorded by the depressing of a signal key which activated a magnetic signal marker on the polygraph. Time in seconds was automatically recorded by the polygraph, given by a G.E. synchronous motor.

The apparatus, with the exception of the polygraph, was set up in a dark, practically sound-proof room. Black cloth screens effectively shielded the apparatus from light leakage so that light came to the subject only through the light tube. All stimuli were de-

livered manually. Timing was obtained from a stop-watch illuminated by a dim, red light.

Eight subjects were used. Six were graduate students or instructors in psychology familar with the general nature of the problem and two were undergraduates naïve as to the problem.

IV. Method

Hudgins' (4) procedure was used with one or two minor changes.

The experiment was divided into three steps involving six experimental periods for each subject. Daily experimental periods of 1-1½ hours were given. Each step occupied 2 periods of experimentation for each subject.

The subjects all received the same number of trials given under the same conditions.

The subject was brought into the experimental room and seated on the stool before the apparatus. The left eye was bandaged so that all light was excluded but free lid-movement was possible. The head was placed in the head rest, and the experiment begun.

Reeves (5) has shown that approximately 4/5 of the total amount of pupillary contraction occurs within the first 3-4 seconds following stimulation by a strong light. For this reason, and also because it was the approximate interval used by Hudgins (4), 4 seconds was chosen as the time for the duration of the stimuli.

Initial Test Procedure. Tests were made for the effect of the various stimuli upon the pupil prior to training. For the initial effect of the buzzer, the subject was instructed as follows:

> Just sit quietly and relax as much as you can. When the light is on fixate the cross hairs. When the light is off, sit quietly in position keeping your eye still and fixated in the general direction of the extinguished light. When the light reappears, again fixate the cross hairs.

The test light was then turned on. When the pupil had reached a comparatively steady state, the buzzer was sounded for 4 seconds. Any resulting pupillary change was followed and recorded.

The instructions for the initial effect of the hand contraction plus the experimenter's verbal commands were:

> Grasp the dynamometer handle with your left hand. When I say "contract" squeeze the handle until it stops. Do not try to force it farther. Hold the handle in position until you

> are told to relax. Be sure to keep the fixation point at all times when the light is on.

The test light was then turned on and when the pupil became relatively steady the command "contract" was given. Any resulting effect upon the pupil was measured.

The instructions for the initial effect of the auto-verbal commands were:

> When the light is turned on, I want you to repeat one of the words "contract" or "relax" but not both during the same trial. Wait until you see the cross hairs clearly and then repeat one of the words.

The test light was turned on and shortly afterwards the subject spoke one of the words. Any resulting effect upon the pupil was measured.

The test light used here was the same as that used in all subsequent tests. From 4-6 tests were taken with each stimulus. Rest periods were given between the tests for the various stimuli.

Conditioning Procedure. Following these initial tests and after a brief rest period, the first step of the conditioning process was begun.

Step 1. Buzzer. This step consisted of 120 trials of paired buzzer and light stimulations. The subject was instructed as follows:

> Just sit quietly and relax as much as you can. Both buzzer and light will be present. When the light appears, fixate the cross hairs. Hold this fixation steadily while the light is on and do not move your eye. When the light disappears keep the eye in position as well as you can so that when the light again comes on it will not be necessary for you to move your eye in order to fixate the cross hairs. Please do not wink while light is on unless it is necessary. Rest periods will be given at intervals.

A typical training trial was as follows: The light and buzzer circuits were simultaneously closed by the experimenter. The duration of the stimulus was 4 seconds. Following this a 10-second period of darkness and silence was allowed to elapse. The trial was then repeated. A 3-minute rest period was given at the end of each 30 trials. A rest period of 5 minutes was given before testing. Tests made at the end of this period failed to show any clear-cut evidence of conditioning. The test light was the same

as that used in the initial tests. On the following day 100 additional trials were given. The final tests for this step were made at the end of these trials. A rest period of 6 minutes was given before testing. Apparent conditioned responses were obtained with but 1 of the 8 subjects. Since this was an attempt to duplicate Hudgins' experiment, the next step was begun whether conditioning was present or not. He reports positive response in 13 out of 14 subjects at this stage, i.e., after 200 trials (4, p. 16).

Step II. Hand responses and verbal stimuli. The subject returned on the third day for a continuation of the conditioning process. The instructions were as follows:

> Grasp the dynamometer handle with your left hand. When I say "contract" squeeze the handle until it stops. Do not try to force it farther. This you will notice activates both the buzzer and the light. Hold the handle in position until you are told to relax. Be sure to keep the fixation point at all times when the light is on. Rest periods will be given at intervals.

A typical training trial was as follows: The experimenter gave the command "contract." The subject responded to this command by squeezing the dynamometer handle until the light and buzzer stimuli appeared. This contraction was maintained for 4 seconds. The experimenter then broke the buzzer circuit and gave the command "relax." The subject responded by relaxing the hand and thus breaking the light circuit. Ten seconds of darkness were then permitted to elapse following which the trial was repeated. Three-minute rest periods were given after every 30 trials. One hundred twenty trials were given. Tests were made at the end of these trials. The following day 100 more trials were given and the final tests for this step were made. Before making these tests a 5-minute rest was given the subject. Apparent conditioned responses to the sound of the buzzer plus the hand contraction were obtained with 3 of the 8 subjects. No responses were obtained to the verbal stimuli alone.

Step III. Verbal stimuli. In this step the procedure was the same as in Step II except that the buzzer stimulus was not used at all. The instructions to the subject were the same as in Step II with the following addition: "Repeat the verbal commands subvocally after you hear me give them." A typical training trial was

as follows: The experimenter gave the command "contract." The subject responded by repeating the word sub-vocally and squeezing the dynamometer handle. The hand contraction was maintained for 4 seconds. The command "relax" was then given. The subject then repeated the command subvocally and relaxed his hand. The hand relaxation broke the light circuit. Ten seconds of darkness elapsed before the next trial. Rest periods of 3 minutes were given after each group of 30 trials. One hundred twenty trials were given. Tests at the end of these trials usually gave evidences of conditioning in all subjects save one. On the following day (6th day) the subject returned and 100 more trials were given. Rest periods of 5 minutes were given before testing.

The tests for the effect of the verbal stimuli alone were made as follows: The subject was told to remove the hand from the dynamometer and to make no movement when he heard the commands. The hand was to remain relaxed in the lap. The test light was then turned on and the pupil measured. The experimenter then repeated one of the verbal stimuli. The pupil was again measured. At this point Hudgins (4) extinguished the test light, allowing the eye to remain in darkness until the next test. In our experiment the test light remained on throughout all tests. The pupil was thus observed not only during the test but also during the interval between tests.

In addition to the tests for the effect of verbal stimuli as given by the experimenter, tests were made without further training for the effect of verbal stimuli as given by the subject. The subject was instructed as follows:

> When the light is turned on wait until you see the cross hairs clearly and then repeat one of the words which I have been using as a signal. Do not repeat both during the same trial. Keep the cross hairs clearly in focus all the time.

The pupil was measured before and after the subject repeated the stimulus word.

Apparent conditioned responses were obtained in 7 out of the 8 subjects following the verbal stimuli when given by the experimenter and when given by the subject.

Tests made for the effect of the stimulus words when repeated subvocally showed no observable conditioned response. In making these tests, the subject was instructed to tap lightly on the table

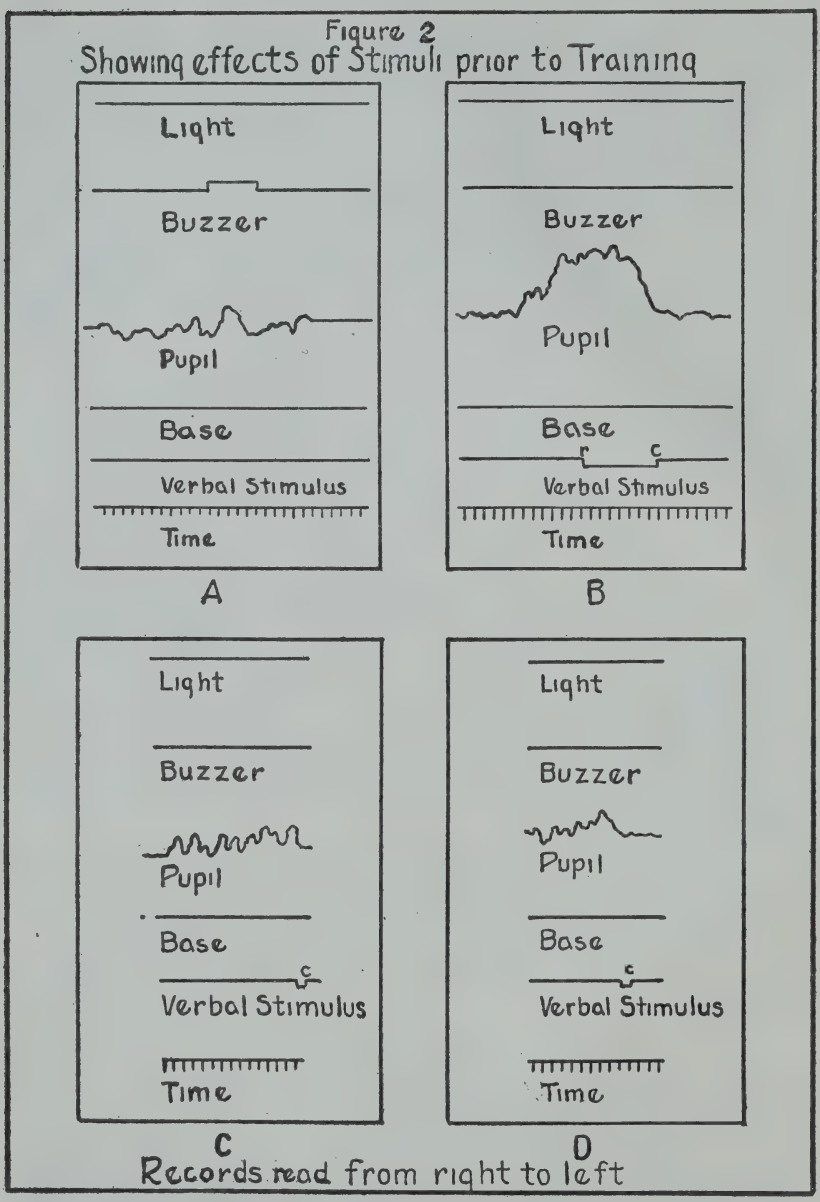
Figure 2
Showing effects of Stimuli prior to Training
Light
Buzzer
Pupil
Base
Verbal Stimulus
Time
A
Light
Buzzer
Pupil
Base
r
c
Verbal Stimulus
Time
B
Light
Buzzer
Pupil
Base
c
Verbal Stimulus
Time
C
Light
Buzzer
Pupil
Base
c
Verbal Stimulus
Time
D
Records read from right to left

FIGURE 2

FIGURE 2

Light—Indicating the presence of the test light.
Buzzer—Any break in this line indicates the incidence of the buzzer stimulus.
Pupil—Tracing of pupillary movements.
Base—Base line.
Verbal Stimulus—Any break in this line indicates the incidence of the verbal stimuli.
Time—In seconds.
A—Response to buzzer.
B—Response to hand contraction plus verbal stimuli.
c—"contract"; *r*—"relax"
C—Response to experimenter's command
c—"contract"
D—Response to subject's command.
c—"contract"

with the right hand when he gave the stimulus. One tap signified "contract," two taps signified "relax."

It will be noticed that the term *apparent* responses has been used throughout. The reasons for this will appear under *Results*.

V. Results

The results of this experiment are of two sorts: (1) Results of the initial test procedure. (2) Results of the conditioning procedure. The data will appear in the form of tracings of the polygraph records. The initial effect of the various stimuli will be considered first.

a. Buzzer. Prior to training the sound of the buzzer was followed in all cases by a dilation of the pupil. A sample record appears in Figure 2, A.[4] It will be seen that the dilation occurred in a somewhat intermittent manner. The dilation persisted for a few seconds but contraction began before the buzzer ceased. This was typical for all subjects. There was no tendency for the pupil to remain dilated while the buzzer was sounding and to begin to contract with the cessation of the buzzer.

b. Hand Contraction Plus Verbal Stimuli. These stimuli also elicited a pupillary dilation in all cases prior to training. A sample

[4]Increased height of the pupil line above the base line signifies dilation, decreased height signifies contraction.

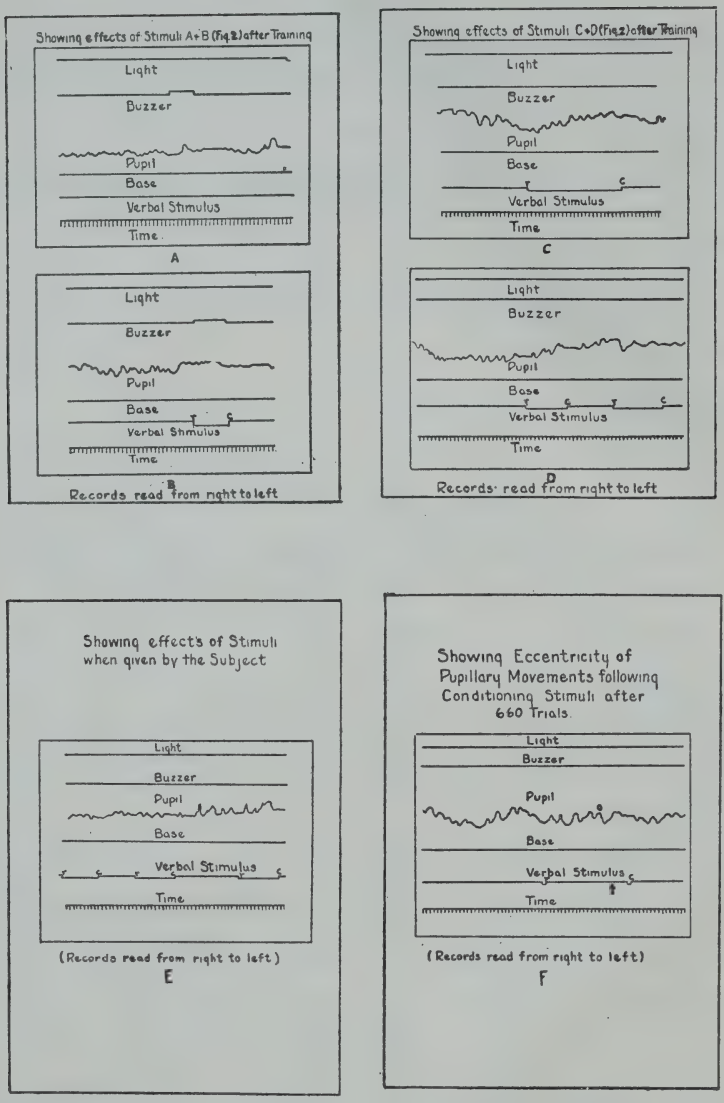
Showing effects of Stimuli A+B (fig.2) after Training
Light
Buzzer
Pupil
Base
Verbal Stimulus
Time
A
Light
Buzzer
Pupil
Base
Verbal Stimulus
Time
B
Records read from right to left
Showing effects of Stimuli C+D (fig.2) after Training
Light
Buzzer
Pupil
Base
Verbal Stimulus
Time
C
Light
Buzzer
Pupil
Base
Verbal Stimulus
Time
D
Records read from right to left
Showing effects of Stimuli when given by the Subject
Light
Buzzer
Pupil
Base
Verbal Stimulus
Time
(Records read from right to left)
E
Showing Eccentricity of Pupillary Movements following Conditioning Stimuli after 660 Trials.
Light
Buzzer
Pupil
Base
Verbal Stimulus
Time
(Records read from right to left)
F

FIGURE 3

FIGURE 3

Light—Indicating the presence of the test light.
Buzzer—Any break in this line indicates the incidence of the buzzer stimulus.
Pupil—Tracing of pupillary movements.
Base—Base line.
Verbal Stimulus—Any break in this line indicates the incidence of the verbal stimuli.
Time—In seconds.

A
Response to buzzer after 225 paired stimulations of buzzer and light. (End of Step I)

B
Response to combined stimuli of buzzer, verbal commands, and hand contraction. Commands given by experimenter. (3rd day)

C
Response to verbal stimuli and hand contraction. Commands given by experimenter. (End of Step II)

D
Response to verbal stimuli alone. Commands given by experimenter. (End of Step III)

E
Response to verbal stimuli when given by subject himself. (Auto-verbal commands)

F*
Response made to verbal stimuli given by experimenter. (End of Step III)

c—"contract"
r—"relax"

*If the test light had been turned out at *"o"* and the command "relax" given at the point where the arrow meets the verbal stimulus line, a relatively clear-cut conditioned response would have been obtained. Particularly would this be true if the tracing of the pupillary movements were amplified 60 times.

record appears in Figure 2, B. The pupillary response exhibits the same general characteristics mentioned before. The dilation occurred in an intermittent manner. The pupil began to contract before the cessation of the stimuli and returned approximately to its former size.

c. Verbal stimuli. The response to the verbal stimuli prior to training was dilation. This occurred whether the stimuli were given by the experimenter or by the subject. Sample records appear in Figure 2, C and D. Figure 2, C, is a record of the response made to the command "contract" given by the experimenter. This record illustrates the difficulty of obtaining numerical values for these responses. Where to measure the magnitude of the re-

sponse is a problem, since the initial dilation is at once followed by a second dilation of similar size. Since such results were typical of this experiment, no attempt has been made to *average* dilation or contraction effects, as was done in Hudgins' (4) experiment.

Figure 2, D, is a record of the response made to the command "contract" as given by the subject. Again the response is a somewhat intermittent dilation returning approximately to normal.

The response to the buzzer after training (220 trials) appears in Figure 3, A. At the onset of the stimulus the pupil began to dilate much in the same manner as it did before training. The direction immediately changed, and a slight contraction took place. Dilation, however, again began before the buzzer ceased to sound. Scarcely had the pupil returned to its former size when another contraction occurred. This second contraction was of approximately the same magnitude and duration as that made directly to the buzzer. The question may arise, which is the conditioned response? Or, is the response an intermittent series of contractions? Such phenomena were current throughout the experiment. They were observed consequent to all stimuli that gave evidence of conditioning. *Therefore contraction effects obtained to the conditioning stimuli have been called apparent conditioned responses.* If, however, the test light had been turned off when the pupil returned to normal following the primary contraction, as was the case in Hudgins' experiments, more clear-cut responses would have been observed. Since this phenomenon was observed in preliminary experiments, the test light was left on throughout all tests. This point marks the main difference between Hudgins' experiments and our own.

The response to the combined stimuli of buzzer, verbal command, and hand contraction after 340 trials appears in Figure 3, B. The reaction here is of the same general type as that obtained to the buzzer alone. While the direct response to the stimuli was *dilation,* contraction took place *following* the cessation of the stimuli. The pupil contracted momentarily and then returned to its former size. This was at once followed by another contraction and a return to "normal." This cycle repeated itself several times throughout the next 17 seconds.

The response to the verbal command and the hand contraction alone after 560 trials appears in Figure 3, C. Here the response is more clear-cut and more closely resembles the results of Hudgins

(4). After a short initial dilation, a rather slow, intermittent contraction took place. Following this the pupil returned to a size slightly greater than the original. This is one of the few cases where possible conditioned dilation effects appeared. The responses in no case exhibited the regularity and preciseness reported by Hudgins. Very probably, even in this record, the dilation would have begun at the time it did whether or not the command "relax" had been given. It is quite possible that here the command was given when the pupil began to dilate. Hence the stimulus became a function of pupillary movement rather than vice versa.

The response to the experimenter's verbal commands alone after 660 trials appears in Figure 3, D. Two samples are shown here. In the first, after a small preliminary dilation followed by contraction and another dilation, a rather pronounced contraction occurred. Following this the pupil dilated to a size slightly larger than its original diameter. Again, however, dilation began *before* the command "relax" was given. The second test gave a rather protracted contraction following the small initial dilation. At the command "relax" the pupil began to dilate but at once contracted further. This contraction endured for about 22 seconds. It was characterized by fluctuation and an intermittent dilation which enlarged the pupil to a size slightly greater than that present at the onset of the stimuli.

At this point, without further training, the pupil was tested for any response made to stimuli given by the subject himself. A sample record appears in Figure 3, E. As usual, at this stage, experimental extinction was already at work. Hence such conditioning effects as were present were usually of the type shown here: rather gross pupillary fluctuations which rapidly decreased as testing continued. The responses in the second test are much smaller than those in the first test. Whether the stimuli were given by the experimenter or by the subject, the response was invariably extinguished in from 4-8 trials.

The records shown above are all taken from the same subject. This subject was the only one who gave evidence for conditioning in every step of the procedure. Possibly, even in this case, too much has been read into the records.[5]

[5]This subject was a male graduate student, age 24, who has a noticeable speech defect but otherwise exhibits no discernible ocular or other neurological imperfection.

Figure 3, F, is a record taken from another subject. This record shows the results obtained at the end of Step III. The stimuli are the verbal commands given by the experimenter. The first break in the verbal stimulus line indicates the command "contract." The second break indicates the command "relax." The interval between the two commands was manifested by rather gross pupillary movements as indicated by the pupil line. Following the command "relax" a dilation took place followed by a rather large contraction of about 9 seconds' duration. This was followed by an intermittent dilation. Such responses were typical for all subjects, where conditioning was apparently present. Again we may raise the question: Which is the conditioned response? Results such as these conduce to hesitancy in calling any of them true conditioned responses.

Using one subject, an attempt was made to establish a conditioned pupillary response directly to verbal stimuli. The verbal stimuli consisted of the sentence, "Now is the time to contract the pupil." This phrase was spoken in such a manner as to occupy approximately 4 seconds. During this time the training light was turned on. The trials were given in the same manner as those involved in the main experiment. This experiment continued for 6 days during which 660 trials were given the subject. Tests failed to show any clear-cut evidence of conditioning. The test record obtained on the 6th day was quite similar to that appearing in Figure 3, E. Hudgins, also using one subject, was unable to establish a conditioned response directly to the verbal commands of "contract" and "relax." He concludes that "hand responses on the part of the subject are essential in establishing the verbal stimuli as conditioned stimuli" (4, p. 48). However, positive results should be possible. There seems to be no reason, logical or psychological, why the nervous system should discriminate between the sound of a buzzer, or the sound of a bell, and the human voice.

VI. Discussion

The results obtained in this experiment are quite different from those reported by Hudgins (4). In our work conditioning did not evidence itself clearly. The responses to the conditioning stimuli after training did not exhibit the regularity reported by him. Experimental extinction was present in every case. There are several reasons for the differences between the results of the two experiments.

1. The two experiments may not have been identical. Possibly Hudgins' method involved some technique not easily duplicated.[6]

2. The results might have been more alike if in this experiment each step of the conditioning procedure had been based on a firmly established conditioned response. Since, however, it was desired to duplicate Hudgins' method as fully as possible, the number of trials given in each step was limited by the number given by Hudgins.

The responses made to the conditioning stimuli in this experiment have been irregular and ambiguous. The evidence for voluntary control of the iridic reflex was slight. Hudgins' (4) hypothesis that voluntary behavior is essentially a conditioned response has not been verified. It does not seem that the use of the pupillary reflex as a ready demonstration of the conditioning process will prove feasible.

The method of observing pupillary movements in these experiments is not the best. Since the experimental procedure forces work under the lowest illumination possible during tests, there is a great possibility for error. Besides the error of apparent visual movement reported above, there is excellent opportunity for movement to be "read into" the pupil. Particularly is this true in the latter period of the testing time. The boundary between pupil and iris rapidly becomes less distinct. Accurate observation requires steady concentration and is definitely a strain on the experimenter. Such opportunity for error increases directly with the decrease in illumination.

To check on this, observations were made by several individuals. All reported accurate measurement to be difficult although possible, provided the experimenter maintains his maximal alertness. Since, however, the test light was already 1 meter candle brighter than that used by Hudgins (4) it was not considered expedient to further increase the test light intensity.

The ideal method for recording pupillary movements in such experiments as this would be photography. If motion pictures could be taken of the eye throughout the conditioning procedure and tests, the results would be much more accurate. Such a method, however,

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[6]An attempt was made to obtain the original draft of Dr. Hudgins' investigation from the Clark University Library, but we were informed that the only available report was the article in this *Journal* (4).

presents several difficulties. It would be too expensive for the average experimenter and laboratory. The problem of illumination would arise. Sufficient intensity to make for clear-cut pictures would probably also contract the pupil to such a degree that further contraction to conditioning stimuli could not be obtained.

Flash-light pictures such as Reeves (5) made could be taken but just when to photograph the eye would present a difficult problem. The sudden illumination would also probably interfere with the conditioning process.

The length of the exposure time seems to definitely rule out the possibility of using infra-red photography, at least at the present time.

The matter of the control of accommodation and convergence demands critical consideration. We took measurements of the pupil while the subjects alternately focussed for distance and on a near object (the lens and its mounting). In no case were the resulting pupillary contractions and dilations as great as those observed consequent to the conditioning stimuli. This observation checks that of Hudgins (4) on the same point. In neither case, however, under the actual conditions of the experiments, can we neglect the influence of the functions of accommodation and convergence upon the iris. Hudgins holds (4, p. 48) that "control experiments indicate that the pupillary responses described could not be due to changes of accommodation and convergence."

It is extremely questionable if the conditioning of the iris is not a pseudo-problem. The sphincter pupillae and the ciliary are supplied by the oculomotor nerve through the ciliary ganglion. The dilator pupillae are supplied by sympathetics from the cells of the superior cervical ganglion, joining the ophthalmic division of the trigeminal and passing to the eyeball by way of the long ciliary nerves. Dilation and constriction are thus served by different neural mechanisms of the most extreme complexity. In view of our present knowledge it would seem safer to say that, when the eyes are brought into position for stimulation, it is impossible to consider *separately* any portion of the concomitant series of very complex events, conventionally designated as accommodation, convergence, adjustment of the iris, and the chemical relation of the rods and cones to the zona pigmentosa of the choroid.

It should be borne in mind that the pupillary size is due to the

tonic state of the two antagonistic mechanisms of dilation and constriction, held in a dynamic equilibrium. Hence the *changes in the size of the pupil may be due either to a contraction of the sphincters or to a relaxation of the radial muscles,* conceivably after the principle of reciprocal innervation. In any such case grave doubt arises as to the accuracy of the concept of a *conditioned* iridic reflex. We should observe with about equal frequency dilation and contraction to the same secondary stimulus. Our records seem to bear out this assumption.

The effective stimulus of the "reflex" adjustment of the iris is of course specified by the combined duration-intensity of the light, by the relative position of convergence of the *two* eyes, by the state of contraction or relaxation of the accessory accommodation musculature (i.e., the extrinsics), and by the state of retinal adaptation (i.e., the chemical state of the visual purple in relation to the wave form of the stimulus light). These functions are certainly not a mass of unit reflexes. The physiologists, for example, prefer to regard them as *associated movements.* Can there be algebraic summation among these functions? Yes, there can be. If this state were true we should expect the same variation in the relative difficulty of conditioning and the same instability of the conditioned iridic repeatedly observed in the various subjects. For all we know, the maximal effect of the secondary stimulus may have been operating upon a portion of the total functioning of the eye not revealed by a change in the size of the pupil. In so far as this is true no crucial experiment has yet been made upon the pupillary response, and future experimentation will have to redirect its efforts so as more properly to take account of the changed conception of the nature of the fundamental factors in the problem, namely, such things as stimulus, unconditioned reflex, etc. This, we believe, is the chief weakness of the conditioned reflex work at present. However, the importance of the problem of demonstrating the most effective mechanism for transforming certain smooth-muscle functions to "voluntary" control stands worthy of the best efforts of all those interested in learning experimentation and theory.

VII. Conclusions

1. The iridic reflex has not proven readily amenable to conditioning and hence does not afford a clear-cut demonstration of

the conditioning process. From the 8 subjects used apparent conditioned responses were observed in but one for all steps. Responses appeared in 3 subjects at the end of Step II and in 7 at the end of Step III.

2. Such conditioned responses as appeared were unstable and transitory. There was no evidence in this experiment that experimental extinction was inoperative.

3. The results of this experiment do not indicate that through a process of conditioning the pupillary reflex may become subject to lasting voluntary control.

4. The conditioned response does not resemble the light reflex. It is much slower and proceeds in an intermittent manner. It does not exhibit the same clean-cut closure that characterizes the pupillary response to a sudden increase in illumination. Hudgins (4) reports the same phenomena.

Such results are to be expected since Hilgard (2), using human subjects, has shown that the conditioned wink reflex is not the same as the response made directly to sound. Wever (7), using the cat, has also shown that the conditioned respiratory response is markedly different from the original response to a shock.

5. The main difference between Hudgins' results and those of the present paper are to be derived from the fact that Hudgins observed and measured the pupil momentarily after the application of the conditioning stimulus. Our observations continued in low illumination throughout the entire testing time. Pupillary movements obscured by the former method are revealed by the latter.

6. A critical examination of the experimental evidence on pupillary conditioning from Cason to Hudgins casts further doubt upon the legitimacy of the concept of a conditioned iridic reflex. The problem of the mechanism of transforming smooth-muscle function to voluntary control, it is hoped, will receive extensive investigation.

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UNE INVESTIGATION DU RÉFLEXE IRIDIEN CONDITIONNEL

(Résumé)

On a répété les expérience de Hudgins sur le conditionnement et le contrôle volontaire du réflexe pupillaire de lumière (ce Journal, 1933, **8**, 3-51), en employant 8 sujets, le même type d'appareil et le procédé identique. Une modification légère de l'appareil nous a permis d'observer et de mesurer le rayon pupillaire dans les intervalles entre les tests. La facilité du conditionnement, la précision et la régularité des réponses, et dans le stage "volontaire" l'absence de l'extinction rapportées par Hudgins n'ont pas été confirmées. L'un de nos huit sujets a montré un conditionnement apparent dans tous les trois stages de Hudgins, trois sujets à la fin du stage II et sept à la fin du stage III. L'évidence n'indique pas que le pupillaire, au moyen du conditionnement, peut devenir sujet à un contrôle volontaire qui dure. On critique le concept du réflexe iridien conditionnel.

Steckle et Renshaw

EINE UNTERSUCHUNG DES BEDINGTEN REGENBOGENHAUTREFLEXES

(Referat)

Hudgins Experimente über das Bedingen der wilklürlichen Beherrschung des Pupillarlichtreflexes (diese Zeitschrift, 1933, **8**, 3-51) wurden mit 8 Vpn. wiederholt. Dieselbe Art Apparat und das identische Verfahren wurden gebraucht. Eine kleine Aenderung des Apparats gestattete uns, den Pupillarradius während der Zwischenzeiten zwischen den Versuchen zu beobachten und messen. Die Leichtigkeit des Bedingens, die Genauigkeit und Regelmässigkeit der Reaktionen, und bei dem "willkürlichen" Stadium die Abwesenheit des Erlöschens, die Hudgins angegeben hat, wurden nicht bestätigt. Eine unserer acht Vpn. zeigte ein deutliches Bedingen bei allen drei Stufen von Hudgins drei Vpn. am Ende Stufe II. und sieben am Ende Stufe III. Das Beweismaterial deutet nicht an, dass der Pupillarreflex durch Bedingen einer dauerhaften willkürlichen Beherrschung ausgesetzt wird. Der Begriff des bedingten Pupillarreflexes wird kritisiert.

Steckle und Renshaw

GALVANIC SKIN PHENOMENA IN EPILEPTICS*[1]

From the Psychological Laboratory of the University of Virginia

J. M. Porter, Jr.

I. Introduction

It has long been the hope of many investigators that in the measurement of apparent electrical resistance of the skin, and its fluctuations when the subject is stimulated in some manner, they had a technique which was to become an important aid in the clinical diagnosis of nervous and mental disorders. A review of the literature dealing with studies of these galvanic skin phenomena in abnormal subjects shows that such a hope is far from fulfillment. And indeed, as a result of certain investigations in which there have been studied the physical and neurophysiological aspects of these phenomena, it is now the opinion of some (11, 2, 13) that these phenomena are to be considered as a part of a more general autonomic response produced whenever stimuli are applied to the body, and in addition that those fluctuations of apparent skin resistance which were termed psychogalvanic reflexes and more lately galvanic skin responses constitute only a valid indicator of the integrity of the sympathetic innervation to those portions of the skin to which the electrodes are attached. Landis (13) has said that in many mental and nervous disorders there is no reason, in view of the physical and neurophysiological findings mentioned above, to expect investigation of these phenomena to show unique or consistent results.

The same process which stripped psychogalvanic phenomena of their psychic attributes and resulted in their being renamed galvanic skin phenomena served also to establish these phenomena as

*Recommended by Frank A. Geldard, accepted for publication by Carl Murchison of the Editorial Board, and received in the Editorial Office, October 9, 1933.

[1]The writer desires to acknowledge his indebtedness to Dr. F. A. Geldard of the University of Virginia, under whose supervision this investigation was performed, to Dr. R. C. Davis of Indiana University for technical advice upon numerous occasions, and to Dr. J. H. Bell, Director of the Virginia State Colony for Epileptics and Feebleminded, for the cooperation which he has extended the writer.

part of a larger group of bodily responses to stimulation, all of which are a function of sympathetic innervation. Therefore, while it may appear extremely improbable that these phenomena will ever possess diagnostic validity, their study as an indicator of sympathetic nervous activity in mental and nervous disorder may still prove of value.

A survey of the literature dealing with galvanic skin phenomena in psychotic individuals reveals that the only research concerned with these phenomena in epileptics was performed by Gregor and Gorn (12) in 1913. They report the results which they obtained upon twelve epileptics who visited their clinic. The purpose of the investigation as a whole was to determine in which of the psychoses the technique of obtaining measures of galvanic skin reaction might be employed as a diagnostic aid. They report, also, results obtained in other types of psychotic individuals, but our interest here is in the results which they obtained on these twelve epileptics. They employed as their measuring instrument a string galvanometer, and all records were taken on "attack-free" days. Concerning the psychotic status of their subjects, Gregor and Gorn merely say that they were said to have had epileptic convulsions. Apparently the degeneracy was slight since none of the patients are reported as institutionalized cases. The stimuli which they employed to elicit the galvanic skin response (psychogalvanic reflex) were pain, olfaction, mental multiplication, and significant free-association words.

Their report of the results obtained leaves much to be desired in the way of clearness. Indeed their general conclusion is that epileptics give very uncertain results. Since the number of cases involved is not large, it is worthwhile to examine them in more detail. In three of their subjects, Gregor and Gorn report that the "rest-curve" was indicative of an emotional reaction on the part of the subject to the situation. By this statement they apparently meant that the subject's total apparent skin resistance tended to decrease during the period of observation. In the nine remaining subjects the "rest-curves" were horizontal. No clue is given as to exactly what Gregor and Gorn meant by a horizontal "rest-curve." From the text one infers that they meant that the curve, representative of the course followed by the subject's total apparent skin resistance, would proceed parallel with respect to its axis of abscissae. The galvanic skin response is reported absent in two of their subjects.

In the remaining ten they say that, though it could be elicited, with one exception, the response was small and irregular. In one of the patients, however, the response is described as being large, approaching that of normal individuals in extent.

The most pertinent criticism to be made of Gregor and Gorn's work is, of course, the failure on their part to report their results in more objective terms. Quantitative data are entirely lacking. Such a deficiency renders it impossible critically to evaluate their results other than as they have done; apparently epileptics give uncertain results. Occasion will be taken later to discuss these results with reference to the results obtained in the present and other investigations.

The object of the present investigation was to study the apparent electrical resistance of the skin and the galvanic skin response in epileptic patients during seizure and non-seizure states. More specifically, it might be said that this investigation was a study of the activity of certain levels in the sympathetic nervous system during seizure and non-seizure states.

II. Apparatus, Subjects, and Procedure

The apparatus employed in this investigation was that described by Davis and Porter (8) for measuring the galvanic reflex. This device makes use of a milliameter to indicate the extent of the galvanic skin response. In addition to its compactness, which rendered it particularly adapted for this investigation, it possesses several further advantages. It has been pointed out in the literature (5) that the extent of the galvanic skin response is a function, in part at least, of the amount of exsomatic current employed. The design of this apparatus is such as to make it possible to vary at will the amount of exsomatic current used, but once set, the amount of current flowing will remain constant. To secure this constant current (d.c.) through the subject, this apparatus uses the principal of a stabilizing vacuum tube described by Davis (4). The exsomatic current is then dependent only on the filament current of the stabilizing vacuum tube, which is regulated by a separate rheostat. In order to secure proportionality of reading to galvanic skin response, the remaining circuit is designed to draw no energy from the subject's circuit. This is accomplished by a particular type of coupling, for the details of which the reader is referred to the

article by Davis and Porter. In addition, the design of this circuit is such that a continuous record of the total apparent skin resistance of the subject can be obtained without the necessity of momentarily placing the subject in a separate circuit. This is important since, due to this fact, the investigator is enabled to obtain a continuous record of the course of the subject's total apparent bodily resistance over a period of time.

The electrodes employed, while particularly adapted for this investigation, have been used in other situations with equal facility. They consisted of two bakelite cups about 1½ inches in diameter and 1 inch deep. The negative electrode had upon the bottom of the bakelite cup a zinc plate to which the negative lead from the measuring apparatus was attached. Upon the zinc plate was a layer of manganese dioxide; the rest of the cup was filled with a saline-soaked cotton. In the positive electrode the manganese dioxide was omitted as unnecessary. The area of contact of such electrodes, when strapped upon the subject, is, of course, approximately constant. Due to the presence of the zinc plate and the manganese dioxide, the chemical reaction under the passage of current in the negative electrode is such as to avoid polarization in both electrodes. A detailed description of the nature of the chemical reaction under the passage of current in the negative electrode has been given by Davis (7, p. 29). When tested by being clamped together and subjected to current flow for 20 minutes, the electrodes showed a change of resistance of only 10 ohms, which may be regarded as an entirely negligible factor in the measurements of values between 10,000 and 100,000 ohms resistance. The leads to which these electrodes were attached were approximately 25 feet in length so that the apparatus would not be pulled from the table by the convulsive movements of seizure subjects. The resistance of these leads may be regarded as an entirely negligible factor.

The subjects studied in this investigation were 70 white epileptic patients at the Virginia State Colony for Epileptics and Feebleminded, located near Lynchburg, Virginia. Thirty-nine were male patients and 31 were female, ranging in age from 19 to 78 years. All manifested some degree of dementia. With a very few exceptions the patients were cooperative when serving as subjects, and the situation for them, if affectively toned at all, appeared to be mildly pleasant. In many instances the patients regarded the pro-

cedure as an attempt to determine "how much sense they had," which worried them not in the least and frequently aroused some amusement on their part.

All experimentation was carried on in the main halls of the different buildings in which the patients were housed. No effort was made to separate the subject from the presence of the other patients for fear of coloring the situation emotionally. Eleven measurements of total apparent skin resistance were made at half-minute intervals on each of the 70 patients studied during the non-seizure state. No patient was studied as a non-seizure subject until at least eight hours after his or her last seizure. In studying non-seizure subjects the patient was seated in a chair near the table upon which the measuring apparatus had been placed. The electrodes were attached to the palmar side of each arm just posterior to the wrist bones. The exsomatic current (0.1 milliampere) was allowed to flow through the subject for an interval of one minute before taking the first measure of total apparent skin resistance, since extremely rapid changes in skin resistance take place at the onset of the current. After eleven such measurements had been made upon the subject, a small phial containing a sponge soaked in a strong solution of ammonia was held under his nose and the instruction given to inhale deeply. This stimulus was administered twice to each subject, record being made of the frequency of incidence of the galvanic skin response.

When the patient being studied was in an epileptic seizure the procedure could not, of course, be so readily standardized. The patient having a seizure was carried as quickly as possible to a position on the floor near the table holding the apparatus. The electrodes were fastened in the usual position as quickly as possible and the current turned on from the apparatus. No appreciable time intervened between turning on the current and taking the first measurement of total apparent skin resistance. It would have been desirable to have allowed the one-minute interval to elapse before taking the first measurement, as was done with non-seizure subjects, but frequently the duration of the tonic and clonic phases of the seizure lasted only a few minutes and it was felt to be more desirable that as much as possible of the seizure epoch be studied. An attempt was made to take records of the patients' total apparent skin resistance at half-minute intervals until such time as the patient

had "cleared" sufficiently to arise from the floor. This was not possible, of course, where the patient passed into a deep sleep upon cessation of the seizure.

Records were obtained on 10 patients, 8 females and 2 males, during the progress of seizures. In several cases more than one seizure epoch was studied in the same patient so that, for purposes of consideration, there are 14 sets of measurements made during the progress of seizures. The stimulus used to elicit the galvanic skin response was given at such time as the investigator felt it would not significantly affect his measurements of total apparent skin resistance, but in every case while the patient was obviously still in the seizure.

For purposes of obtaining comparative data a procedure identical with that employed with patients in the non-seizure state was used with a control group of 17 college students. This group was composed of students of both sexes ranging in age from 20 to 37 years.

III. Results

1. *Galvanic Skin Phenomena in Epileptic Patients During Non-Seizure States.* Measurements were made of total apparent skin resistance on each of the 70 non-seizure subjects at half-minute intervals during a five-minute period of observation. These eleven measurements of apparent skin resistance obtained on each subject were averaged to obtain a value, a quite arbitrary one, it is true, representative of that individual's apparent skin resistance during the period of observation which could be used for purposes of comparison. The value which is presented as the average apparent skin resistance for a group of patients was obtained by averaging the values obtained for each patient in the group by the procedure described above.

Since previous investigators have demonstrated that no significant sex differences are to be found in galvanic skin phenomena when normal individuals are employed as subjects, the data of this investigation were first analyzed in order to determine whether or not sex differences existed among the subjects studied. The average total apparent skin resistance for the group of 39 male patients was 75,025 ohms, with a standard deviation of 29,808 and a range of individual averages from 130,000 to 15,000 ohms. The average total apparent skin resistance for the group of 31 female patients

was 51,064 ohms, with a standard deviation of 20,031 and a range of individual averages from 110,000 to 18,000 ohms. The difference between the average total apparent skin resistance of the male patients and of the female patients is approximately 24,000 ohms and is a statistically reliable difference $\left(\frac{D}{\sigma_{diff.}} = 4.013\right)$.

The average total apparent skin resistance for the control group was 50,647 ohms, with a standard deviation of 15,834 ohms. In this group individual averages ranged between 82,000 and 23,000 ohms. The difference between the average total apparent skin resistance of the male patients and the control subjects was slightly over 24,000 ohms and is a statistically reliable difference $\left(\frac{D}{\sigma_{diff.}} = 3.983\right)$; while the difference between the average total apparent skin resistance of the female patients and the control group was less than 1,000 ohms and does not represent a real difference $\left(\frac{D}{\sigma_{diff.}} = 0.078\right)$.

The course followed by the average total apparent skin resistance of each group during the five-minute period in which readings were taken is shown in Figure 1. Upon inspection it will be seen that for all three groups the curves have the same general form, being characterized by a negatively accelerated slope. The form possessed by each of these three curves is more or less typical of the curves obtained by other investigators who have dealt with groups of normal subjects. Three hypotheses have been advanced in explanation of this negatively accelerated initial decrease of apparent skin resistance in normal subjects. Such a diminution in skin resistance may be due to the situation in which the subject finds himself possessing an affective tone. Farmer and Chambers (10) have, how-

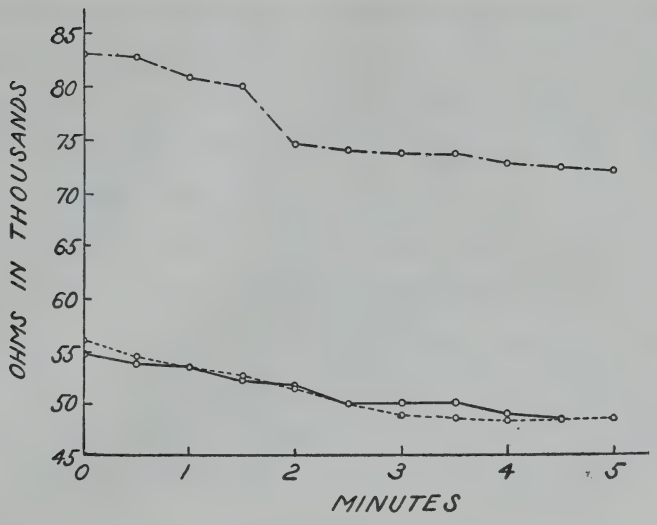

FIGURE 1

THE COURSE OF APPARENT SKIN RESISTANCE IN NON-SEIZURE AND CONTROL GROUPS

— - — male epileptic patients; - - - - - - female epileptic patients; ——— control subjects.

ever, termed this initial diminution of resistance the curve of physical accommodation. They regard it as due to incomplete ionization in the skin under the electrodes. The diminution continues, they say, until complete ionization is attained. The length of time required to reach this equilibrium varies from less than one to more than fifteen minutes and is a function of the concentration and nature of the electrolyte used in the electrodes. It has been pointed out (6) that this initial decrease in bodily resistance may also be regarded as due to the penetration of the electrolyte into the skin under the electrode. It is not necessary to attempt a critical evaluation of these three hypotheses here. The significant point is that the same initial diminution occurs in epileptic as well as normal subjects.

It is not possible at the present to offer a conclusive explanation for the fact that a sex difference as regards total apparent skin resistance was found among the epileptics. This sex difference may

possibly be due to one or more of several factors, consideration of which will be taken up in a later section.

The galvanic skin response was elicited in 22 of the 39 male patients; with the female patients the frequency of incidence is higher, 23 of the 31 patients in this group responding to stimulation; in the control group, 14 of the 17 subjects reacted to stimulation. Expressed as percentages, 56% of the male group, 74% of the female group, and 83% of the control group reacted to stimulation. These differences are not statistically reliable. With regard to the ohmic extent of these responses, as well as the duration of the latent and recovery periods, the data unfortunately do not permit quantitative comparisons. It is the opinion of the investigator, however, that no significant differences were manifested.

2. *Galvanic Skin Phenomena in Epileptic Patients During Seizure States.* Those who have attempted to observe some aspect of the epileptic seizure will readily appreciate the difficulties to be encountered. The epileptic seizure, except in a very few instances, is an exceedingly transitory form of behavior. Its onset is, in most cases, without warning and its duration, from a few seconds to many hours. However, it is only in the case of a severe *status epilepticus* that its duration will exceed a very few minutes, hence the time in which aspects of the seizure may be studied is extremely short, especially where some preparation is necessary before observations can begin, as was the case in this investigation. The time required to reach the patient and move him close enough to the apparatus to make possible attachment of the electrodes was usually sufficiently long to make it impossible to obtain measures of the apparent skin resistance during the tonic phase of the seizure. In addition, epileptic seizures are not usually produced upon demand. The only procedure productive of results is for the investigator just to sit and wait. There are said to be ways of inducing seizures in a wholesale manner, but these methods, it must be added, are usually found to be in disfavor with the attending institutional staff for obvious reasons. By patiently waiting over a period of time, records were obtained during the progress of fourteen seizures. Of course the frequency of seizures among the patients during this period of time was larger, but the fact that an appreciable length of time is required to prepare the subject for the procedure of measuring apparent skin resistance serves to make it impossible to use

seizure subjects as such unless they maintain that status for at least two or three minutes, and in a great many cases the duration of the seizure fell short of this period.

Measures of total apparent skin resistance, as well as records of galvanic skin responses, were obtained upon ten of the patients, eight females and two males, during the course of seizures. Since quite an appreciable length of time was required to move the seizure subject close to the apparatus and attach the electrodes firmly, the investigator was unable to obtain records of skin resistance from the very onset of the seizure through its entire course. In all but one case, reference to which will be made later, the tonic phase of the convulsion had passed before the electrodes had been attached. Since some of the patients were subject to frequent seizures, in two cases 2, and in one case 3, records of bodily resistance and the galvanic skin responses were obtained. The data contain, therefore, 14 such records. These seizure records were treated in the same manner as the non-seizure records to obtain individual and group averages.

For purposes of comparison there is presented in Table 1 the

TABLE 1

Subject number	No. of records obtained while in seizure	Apparent skin resistance in ohms	
		Non-seizure state	Seizure state
1	1	57,000	55,180
2	1	55,090	43,363
3	3	53,318	54,014
4	1	110,453	27,100
5	1	54,090	42,423
6	2	60,454	29,400
7	1	38,890	28,633
8	1	50,818	45,240
9	1	47,000	29,494
10	2		38,468
	Average	58,568	39,332

average total apparent skin resistance during non-seizure and seizure states for each individual in this group of seizure subjects.

No non-seizure record was obtained on subject number 10 because it so happened that she was not available as a non-seizure subject under the criterion laid down in a previous section for the selection of this group of subjects. Upon one occasion while the

investigator was talking to subject number 3, he surmised from her behavior that she would shortly go into a seizure. The electrodes were immediately strapped upon her wrist and the exsomatic current turned on. This was the sole case in which measurements were obtained on a seizure subject during the tonic phase of the convulsion. In this instance the subject's resistance during the tonic phase was extremely high in comparison with measurements made upon this same subject during other seizures. The initial measure of apparent skin resistance during the tonic phase was 94,000 ohms, while the initial measurements upon two other occasions taken during the clonic phase were 68,000 and 48,000 ohms. It is, of course, possible and probable that daily variations in skin resistance may be responsible for the high value obtained in the above instance.

Reference to the values given in Table 1 shows that with the exception of subject number 3, and possibly subject number 10, the

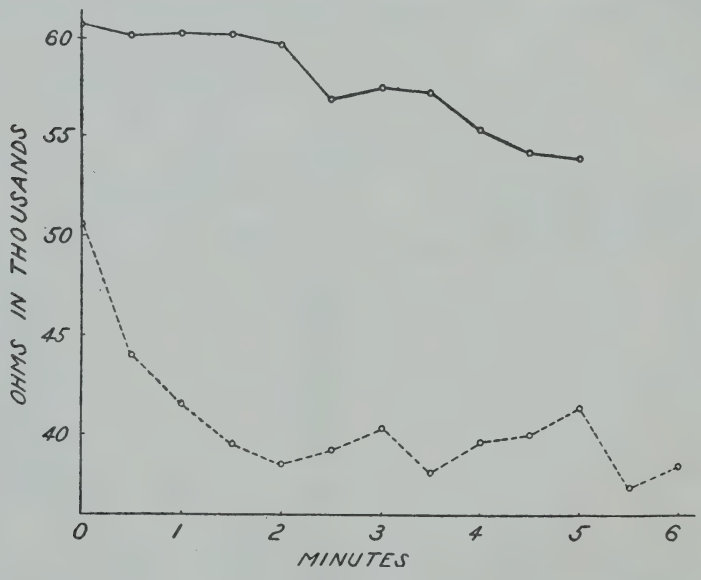

FIGURE 2

THE COURSE OF THE AVERAGE APPARENT SKIN RESISTANCE OF A GROUP OF TEN EPILEPTIC PATIENTS DURING NON-SEIZURE AND SEIZURE STAGES

———— course of average apparent skin resistance during non-seizure state; - - - - course of average apparent skin resistance during progress of seizures.

total apparent skin resistance of all seizure subjects is lower during a seizure state than during a non-seizure state. This fact is more vividly illustrated by the curves in Figure 2. The broken-line curve in this figure portrays the course of the average total apparent skin resistance of this group of ten patients during the seizure epoch. For purposes of comparison the course of the average total apparent skin resistance for this same group during a non-seizure state is portrayed by the solid-line curve. The seizure curve has purposely been shortened. Due to variation in length of the seizure epoch from patient to patient, the data obtained were such that after six minutes the form assumed by this curve is not typical of the form of the individual curves of which it is composed. The curves in Figure 3 were plotted from several individual records selected purely at random and are presented for comparative purposes.

While taking measurements of total apparent skin resistance upon the seizure subjects, the investigator attempted to note beside each such measurement any change in the behavior of the subject. Where it was possible for him to do this it usually amounted merely to

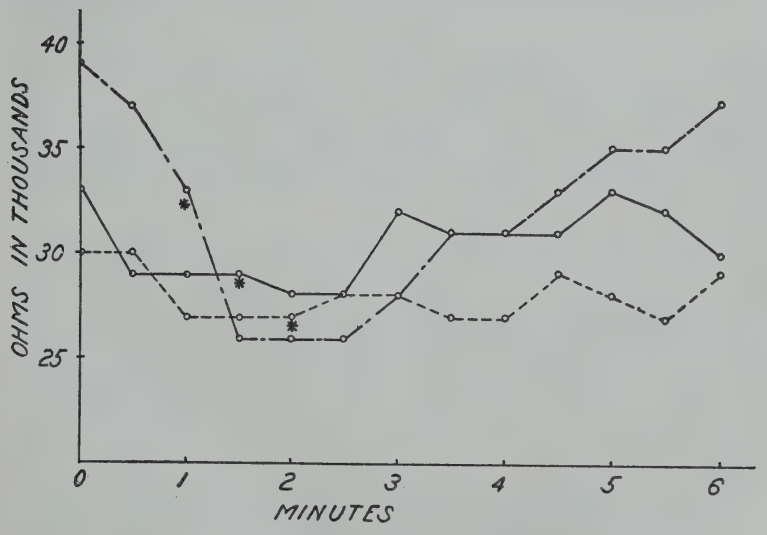

FIGURE 3

THE COURSE OF APPARENT SKIN RESISTANCE DURING A SEIZURE PLOTTED FROM THREE INDIVIDUAL RECORDS

*The point at which the clonic phase of the seizure appeared to cease.

the noting down of the point of cessation of the clonic convulsions. This point of cessation has been indicated upon the curves in Figure 3. The curves presented in Figures 2 and 3 show that during the seizure epoch the apparent skin resistance decreases rather sharply during the first two minutes or so, and then, after varying intervals, begins to rise. The curves in Figure 3 serve to show also that the greatest decrease in skin resistance takes place during the clonic phase, and that in general it is at the cessation of the clonic convulsions that we find the lowest apparent skin resistance. Inasmuch as it is at this point, the cessation of the clonic convulsions, that epileptologists regard physical fatigue to be at its highest, the statement that minimum apparent skin resistance occurs approximately at the point of maximum physical fatigue seems justifiable.

The negative slope of the curves in Figures 2 and 3 which portray the course of skin resistance during seizures is undoubtedly accentuated to some degree by the effect of whatever process makes for what Farmer and Chambers have termed the curve of physical accommodation. As has been pointed out before, with seizure subjects the customary interval of one minute was not allowed to elapse before the initial measurement of skin resistance was taken. The extent to which the effect of this process of accommodation would serve to accentuate the negative slope of these curves is not known. Comparison of the seizure curves with the non-seizure does serve to indicate, however, that the rapid decrease in skin resistance portrayed in the former is due, to a great degree, to the effects of the seizure.

A suggestion as to the nature of that aspect of the seizure responsible for the decrease in apparent skin resistance is given by the following incident. Among the patients at the Colony was a feebleminded girl who was able perfectly to simulate an epileptic seizure. This girl was not an epileptic, but the seizures which she simulated were very realistic and typical. She has upon several occasions deceived experienced observers who were not familiar with her history. During the first day of the investigator's stay at the Colony some one on the ward suggested to her that she have a seizure for his benefit. At the time of taking the measurements of apparent skin resistance and testing for the galvanic skin response, the investigator knew nothing of her past history. Her average total apparent skin resistance in this "seizure" state was 14,000 ohms, in a non-seizure

state it later proved to be 50,000 ohms. If the course of her apparent skin resistance during the "seizure" state is plotted graphically, as was done with the seizure subjects, the curve displays the same characteristic initial decrease with a negative acceleration, the point of minimum resistance occurring soon after cessation of the convulsive movements. In this case it seems certain that this lowered average total apparent skin resistance, as well as the manner in which it decreased during the "seizure," is attributable to the effects of the violent muscular reactions undergone by the patient. The decrease in total apparent skin resistance, as well as the manner in which it decreased, in the case of the seizure subjects observed is conceivably due to this same cause.

The olfactory stimulus which was designed to arouse the galvanic skin response was administered only during the clonic phase of the convulsive seizure. This was more a matter of necessity than design, since to have given the stimulus immediately upon onset of the seizure might have materially affected the course of the subject's apparent skin resistance. In all of the subjects studied, however, the stimulus was administered while the subject was still obviously in seizure.

Only one subject failed to manifest the galvanic skin response consistently while in a seizure. In two of the remaining nine the response was not consistently given to stimulation, but the frequency with which it was given was sufficiently high to justify the conclusion that its manifestations were not artifacts of the experimental situation but were actually galvanic skin responses to stimulation. In the remainder of this group of subjects the galvanic response appeared consistently to stimulation during the progress of the seizure.

As a rule, in this group of subjects the ohmic extent of the galvanic skin response given while the subject was in a seizure was small as compared with the extent of the responses given by the same subject in a non-seizure state.

IV. Comments

The horizontal "rest-curves" which Gregor and Gorn obtained in nine of their twelve subjects were not found to be present with such a high frequency among the records obtained in this investigation. In only two or three instances did it happen that the course

followed by the subject's apparent skin resistance proceeded parallel to the axis of abscissae throughout the period of observation. In the large majority of the cases the apparent skin resistance, with that negative acceleration which is typical of curves portraying apparent bodily resistance, decreased in no groups of normal subjects. In a few cases the apparent skin resistance increased, but such instances were not sufficiently numerous to significantly affect the form of the curve representative of the groups. It may be said then that in epileptics during a non-seizure state the apparent skin resistance displays the same initial negatively accelerated diminution as is found when groups of normal subjects are used. Davis (7) has investigated this phenomenon in six different groups of subjects, each group being composed of subjects socially distinct from those of the other groups, and finds that in each group the curve which portrays the course of apparent skin resistance shows this same form. The three hypotheses which have been advanced in explanation of this initial diminution have already been mentioned in a previous section.

The discrepancy between the "rest-curves" obtained by Gregor and Gorn and the comparable curves obtained in this investigation (Figure 1) is, the writer feels, most probably a result of the procedure or apparatus employed by the former investigators. A greater number of patients were studied in the latter investigation, hence the results obtained should be more reliable, granting equal validity to the apparatus employed in each instance. And indeed there is, in the writer's opinion, no reason why the course of apparent skin resistance in epileptics should differ from the course followed in normal subjects, since in the opinion of most investigators this diminution is a result of a process of ionization, which is a function of such purely physical factors as the strength of the exsomatic current employed and the concentration of the electrolyte used in the electrodes. That the form of these curves is not an artifact of the procedure or apparatus employed in this investigation is amply substantiated by the amount of variation in form shown by the curves of individual records.

In view of the various bodily processes which it has been shown to reflect, there is no reason to expect that the epileptic's apparent skin resistance should fail to show significant variation from that of normal subjects. This does not necessarily imply that such variation is due to neural changes within the body as a result of the

psychoses of the patient, but rather that such variation is a result of a change in one or more of the bodily processes with which apparent skin resistance has been shown to be in relation. Darrow (3) found that thermal and resistance curves obtained simultaneously from the same skin area are almost identical in appearance. Upon the basis of this evidence and that of Wang and Lu (18), and others who have studied the relation between the activity of the sweat glands and galvanic skin phenomena it has been suggested that changes in apparent skin resistance reflect changes in the temperature regulatory mechanism of the body. Later Purdy, Johnson, and Sheard (15, 16) found a relation between body potential and basal metabolism. Body potential was measured from the back of the wrists and such measures are comparable to those of apparent skin resistance. The relationship which they found is approximately a logarithmic one, higher potentials being accompanied by lower basal metabolic rates. If the subject's circulation is normal, they say that in 62% of the cases the metabolic rate may be calculated within 4 points by the formula $x = \frac{\log y - \log 0.005}{-0.0396}$, where x is the basal metabolism and y is the difference of electrical potential of the skin. And, finally, as has been mentioned before, these phenomena are regarded by Landis and by Richter as indicative of the functional integrity of the sympathetic nervous system.

The fact that epileptics, as a group, present a higher apparent skin resistance than do normal subjects tends towards a rough correspondence with the facts that are given in the literature concerning basal metabolism in epileptics. Lennox and Cobb (14) report that the majority of adult patients with convulsions have either a normal or reduced rate of oxygen consumption. Du Bois (9, pp. 297, 299) gives the figures obtained in two instances: in the first it was reported that of 22 epileptics studied all had basal metabolic rates ranging between —20 and +10; the second report was that in 24 cases of epilepsy observed 84% had metabolic rates ranging between the same limits. If in this investigation the apparent skin resistance of all the epileptics had been averaged with no differentiation as regards sex, the value obtained would have been lower, approximately 63,000 ohms in fact. This suggested relationship between basal metabolism becomes more probable when this figure for

the entire group of epileptics is compared with the 51,000 ohms obtained with the control group.

While it may be possible that the increased apparent skin resistance found in epileptics is a reflection of a diminished basal metabolic rate this same explanation does not account for sex difference. Lennox and Cobb make no differentiation in the basal metabolic data which they give as regards sex. On the other hand, Du Bois (9) reports that in normal individuals women have a slightly lower rate of basal metabolism than men. If this relative difference between the sexes is maintained in epileptics we should expect the female patients to have the higher apparent skin resistance. The fact that such is not the case makes it worth while noting the following possibilities. The frequency of seizures among females is noticeably higher than among the male patients. Kauffman (9, p. 407) studied the basal metabolism during an epileptic seizure and found it greatly increased. Bornstein (9, p. 407) thought that the increase in metabolism lasted for many hours after the seizure, but later work has shown that such is not always the case. Hence it may be said that there exists some probability that the fact that apparent skin resistance is lower in female patients is due to the greater frequency of seizures among them. Such a view is only a tentative one however.

The decrease in apparent skin resistance during the seizure epoch is in correspondence with the results of an investigation by White (19), who reported that the expenditure of muscular energy is accompanied by a decrease in skin resistance. This decrease during the seizure is also in agreement with the findings of Kauffman concerning metabolism during the seizure, according to the findings of Purdy, Johnson, and Sheard. It is to be anticipated also according to evidences demonstrated by Richter (17) to the effect that a low apparent skin resistance is indicative of sympathetic activity, since according to Cannon (1) the autonomic nervous system would be expected to be functioning during such crises as these in the behavior of the organism.

The incidence of the galvanic skin response serves to prove the integrity of certain levels of sympathetic innervation in epileptics during seizure and non-seizure states, though on the basis of these data nothing can be concluded concerning this response during the tonic phase of the convulsion. The differences in frequency of incidence betwen the three groups are not statistically reliable and may be due to sampling.

Both the low apparent skin resistance and the presence of the galvanic skin response during the seizure may have some tentative inference for theories concerning the neural mechanism underlying the seizure. Of the possible theories, which are fully discussed by Lennox and Cobb, the data of this investigation, in the opinion of the writer, would tend to substantiate that type of theory which hypothesizes a severance of the lower motor levels from the inhibitory influence of the cortex and so permits these lower levels to initiate the violent motor activity characteristic of the seizure. It seems conceivable that if one regards the seizure as originating with a cortical motor discharge that some of the released energy would find its way out over the cranial branch of the autonomic and thus result in an inhibition to some degree of sympathetic activity, which condition should be reflected in a heightened apparent skin resistance and an inhibition of the galvanic skin response. Though some evidence exists for the latter, the former is certainly not the case. The writer is inclined to regard the above hypothesis as merely tentative and in no manner substantiated by the data of this investigation.

V. Summary

1. Apparent skin resistance and galvanic skin responses were studied in a group of 39 male and 31 female epileptic patients in a non-seizure state. Measures of apparent skin resistance were made at half-minute intervals during a five-minute period on each of these subjects, at the end of which time the subject was tested for the galvanic skin response by being stimulated olfactorily.

2. From the data thus obtained the average apparent skin resistance for this five-minute period was computed for each subject, and from these figures an average for male and female groups determined. In addition there were computed group averages for each of the eleven half-minute intervals. The latter of these averages was used to portray the course of the apparent skin resistance during the five-minute period of observation (Figure 1).

3. The procedure described in the preceding paragraphs was carried out for a group of 17 normal individuals to supply comparative data.

4. A one-minute interval was permitted to elapse between the onset of the exsomatic current and the taking of the first measure of apparent skin resistance. Subsequent to this interval the ap-

parent skin resistance of the epileptic patients, as a group, manifested the negatively accelerated diminution typical of the records obtained from groups of normal subjects under the same procedure. It was pointed out that, in view of the processes generally assumed to be responsible for such a decrease in apparent skin resistance, there was no reason to expect that epileptics would show any significant variation in this respect.

5. The average apparent skin resistance of the epileptics was found to be higher than that of the control group. In addition, a sex difference was found to exist in this respect among the epileptic patients, the apparent skin resistance of the male patients being significantly higher than that of either the female patients or the control group. The resistance of the female patients was found to be higher than that of the control group, but the difference is not statistically reliable.

6. The frequency of incidence of the galvanic skin response to olfactory stimulation was found to be lower in the group of non-seizure subjects than in the control group. These differences, however, are not statistically reliable.

7. Apparent skin resistance and galvanic skin responses were studied in ten patients, two males and eight females, during the progress of epileptic seizures. Due to the length of time required to prepare the subject for observation records were not obtained of these phenomena during the tonic phase of the seizures, with one possible exception. Measurements of apparent skin resistance were made from the onset of the exsomatic current, it not being feasible to permit the intervening one-minute period as was done with non-seizure subjects, at half-minute intervals until such time as the subject had cleared sufficiently to arise from the floor or had passed into a state of coma resembling sleep. The subject was stimulated for the galvanic skin response at no set time during the progress of the seizure, save that stimulation was administered while the subject still was in seizure. Due to the fact that several patients had seizures with more frequency than others it was possible to obtain in all fourteen records for study.

8. During the seizure apparent skin resistance was found to decrease with a negative acceleration, reaching its lowest value approximately with the cessation of the clonic phase of the seizure. The form of the curves portraying the course of apparent skin re-

sistance during the progress of the seizure (Figures 2 and 3) is probably complicated, it was pointed out, by the process of ionization under the electrodes, generally assumed to be responsible for the initial diminution of apparent skin resistance in normal subjects. Especially is this apt to be true since no time was permitted to elapse between the onset of the exsomatic current and the first determination of apparent skin resistance. The slope of these curves is such, however, as to render it improbable that their form is significantly altered by this process of ionization.

9. The average apparent skin resistance of these subjects was found to be much lower during the progress of the seizure than during a non-seizure state (Table 1). This fact is to be expected on the basis of the enormous expenditure of muscular energy during the seizure as well as variations in other bodily processes said to be reflected by changes in apparent skin resistance.

10. Galvanic skin responses were manifested to stimulation in every subject but one, though they tended in the opinon of the writer to be less in ohmic extent than those manifested during non-seizure states.

11. It was pointed out that the data of this investigation tended to correspond with the relation found by Purdy, Johnson, and Sheard between body potential (comparable to electrical skin resistance measures) and basal metabolism as well as the facts of basal metabolic rate in epileptics as given by Lennox and Cobb and by Du Bois. In addition, it was suggested that these data would tend toward substantiating theories of the neural mechanism underlying the seizure epoch which posit a severance of cortical inhibition over lower motor levels as the cause of the seizure, rather than theories which hypothesize an intense cortical motor discharge as the activity initiating the seizure.

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LES PHÉNOMÈNES GALVANIQUE DE LA PEAU CHEZ LES ÉPILEPTIQUES

(Résumé)

On a étudié la résistance apparente de la peau et les réponses galvaniques de la peau chez un groupe de 39 mâles et de 31 femelles épileptiques pendant les états de non-accès. On a étudié aussi ces mêmes phénomènes chez un groupe de 8 femelles et de 2 mâles épileptiques pendant le progrès des accès épileptiques. Un groupe de contrôle de 17 sujets normaux a fourni

des données comparatives. Chez le groupe des sujets de non-accès on a constaté que la résistance apparente de la peau pendant la durée de cinq minutes où on l'a observée a présenté la diminution négativement accélérée typique des données obtenues chez les groupes de sujets normaux étudiés selon la même méthode de processus. La résistance apparente moyenne de la peau chez le groupe d'épileptiques a été plus élevée que celle du groupe de contrôle. La fréquence de l'incidence de la réponse galvanique de la peau est moins élevée chez le groupe épileptique que chez le groupe de contrôle mais la différence n'est pas constante. Chez les sujets étudiés pendant les états d'accès on a observé que la résistance apparente de la peau a diminué avec une accélération négative, arrivant à sa plus petite valeur approximativement avec la cessation de la phase convulsive de l'accès. La résistance apparente moyenne de la peau chez les sujets d'accès a été moins élevée pendant le progrès d'un accès que dans un état de non-accès. Des réponses galvaniques de la peau ont été manifestées par tous les sujets étudiés dans les états d'accès, à l'exception d'un sujet, quoique ce soit l'opinion de l'expérimentateur qu'elles ont tendu à être d'une plue petite étendue ohmique que celles manifestées pendant les états de non-accès.

PORTER

GALVANISCHES HAUPTPHAENOMEN BEI EPILEPTIKERN

(Referat)

Der scheinbare Hautwiderstand und die galvanischen Hautreaktione wurden bei einer Gruppe von 39 männlichen und 31 weiblichen Epileptikern während der Zeit zwischen Anfällen untersucht. Dieselben Erscheinungen wurden auch bei einer Gruppe von 8 Patienten und 2 Patientinnen während eines epileptischen Anfalls untersucht. Eine Kontrollgruppe von 17 normalen Menschen lieferte das Vergleichsmaterial. Bei der Gruppe in der Zeit zwischen Anfällen ergab sich, dass der scheinbare Hautwiderstand während der 5 Minuten Zwischenzeit, in der er beobachtet wurde, eine negativ beschleunigte Abnahme darstellte, die typisch für die Reaktionen der normalen Vpn. unter denselben Umständen ist. Der scheinbare Durchschnittshautwiderstand der Epileptiker war höher als bei der Kontrollgruppe. Der Häufigkeitswert der galvanischen Hautreaktion ist neidriger bei den Epileptikern als bei der Kontrollgruppe, aber der Unterschied ist nicht zuverlässig. Bei denjenigen Patienten, welche während der Anfälle untersucht wurden, stellte sich heraus, dass der scheinbare Hautwiderstand mit einer negativen Beschleunigung abnahm, und erreichte seinen niedrigsten Wert ungefähr mit dem Aufhören des klonischen Stadiums des Anfalls. Der scheinbare Durchschnittswiderstand bei den Anfallpatienten war niedriger während des Verlaufs eines Anfalls als während der Zeit zwischen Anfällen. Galvanische Hautreaktionen wurden bei allen Vpn. ausser einer beobachtet, die bei einem Anfall untersucht wurden, obgleich es die Meinung des Verfassers ist, dass sie neigten, weniger an Ohmumfang zu sein als bei derjenigen, die während der Zeit zwischen Anfällen erschienen.

PORTER

STUDIES OF INHIBITIONS IN CONDITIONED RESPONSES* [1] [2]

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These studies are concerned with the development of certain inhibitions of conditioned responses. Two types of inhibition, both encountered by Pavlov (4) in his experiments on dogs, were studied in humans. One of these, which Pavlov terms *conditioned inhibition,* is the inhibitory capacity gradually acquired by an extraneous stimulus after repeatedly accompanying a conditioned response which is in a partial or complete inhibitory state. The other, which he terms *secondary extinction,* is the inhibitory effect produced upon one conditioned response as the result of rendering another one inhibitory through repeated unreinforced evocation. The object of the present experiments was to make a quantitative study of these phenomena in humans.

APPARATUS,[3] SUBJECTS, AND GENERAL PROCEDURE

The eyelid reaction, because of the readiness with which it lends itself to conditioning and the relative ease with which it can be evoked and recorded, was selected for the basic reaction in these studies of conditioning.[4] No attempt was made to study latencies and other temporal relations as this had already been done in connection with the eyelid reaction with extreme precision by Hilgard (2). In the present studies temporal relations were disregarded and only relative measures of amplitude were sought.

*Accepted for publication by Clark L. Hull of the Editorial Board and received in the Editorial Office, January 29, 1934.

[1]Condensed from a dissertation presented to the Faculty of the Graduate School of Yale University in partial fulfillment of the requirements for the degree of Doctor of Philosophy (7).

[2]The writer wishes to express his appreciation to Professor Clark L. Hull under whose direction these studies were carried out.

[3]Much of the present apparatus was used in an experiment previously reported by the writer (8).

[4]Investigations of the conditioning of this reaction have been reported by Cason (1), Hilgard (2), and Switzer (9).

A modified form of the apparatus employed by Switzer (9) was used. The subject, seated in a slightly reclining position in a Morris chair, was surrounded from the waist up by a blackened hood which served the threefold purpose of (1) diminishing the visibility of the padded hammer used to evoke the wink, (2) reducing eye strain, and (3) preventing any visual cues that might unintentionally be given by the experimenter. To further these effects the room was somewhat darkened by drawing the shade.

A small electric light dimmed by a layer of black sateen so as to produce at most only a very minimal wink, fastened inside the hood approximately 2 feet directly in front of and on a level with the eyes of the subject, served as one extra stimulus. The sound of an electric buzzer, damped so as to produce at most only a minimal wink, was used as another extra stimulus.

For eliciting the unconditioned wink a spring-driven padded hammer provided with an electromagnetic release was allowed to produce a sudden rap on the right cheek just below the eye. The hammer and releasing mechanism, which were fastened inside the hood, were blackened so as to reduce their visibility. Cocking of the hammer was accomplished by means of a thread pulled manually by the experimenter.

Recording of the winks was accomplished by means of a finely drawn glass needle in contact with the vertically running smoked paper band on an extension kymograph. The glass needle was activated by a fine human hair which was run from its point of attachment on the needle to the eyelid of the subject to which it was fastened by a bit of adhesive cement. Tension on the thread was regulated by vertical adjustment of the needle.

Recording of the various stimulus combinations was accomplished by the use of the electromagnetic signal markers placed in parallel circuit with the several stimulating devices. The current used to operate the stimulating devices and their markers was taken from a 6-volt storage battery. The stimuli were activated automatically by adjustable motor-driven switches in an adjoining room. The components of any stimulation were determined before automatic completion of the circuit by means of switches operated by the experimenter in the room with the subject.

The subjects were all male college students.[5] They were in-

[5]Those subjects who showed excessive spontaneous blinking were eliminated before critical tests were made.

structed at the beginning of each period to keep their eyes open and looking straight ahead and to hold their heads as still as possible.

Conditioned Inhibition

The term *experimental extinction,*[6] as used by Pavlov (4), is generally familiar. It signifies progressive weakening of a conditioned reaction from successive repetition without reinforcement by the unconditioned stimulus. Conditioned inhibition, which is also discussed at some length by Pavlov, is essentially conditioned extinctive inhibition, as will be revealed by the customary method of its establishment. The method is this. After a neutral stimulus has been rendered a conditioned one, an additional stimulus is repeatedly presented in combination with it. (It is customary for the additional stimulus to precede slightly the conditioned one.) During this process the conditioned stimulus, without the additional one, is frequently reinforced. The result is that it behaves in two completely different ways. When presented alone it produces its response; when preceded by the additional stimulus it produces none. What apparently takes place is that the additional stimulus, being presented with the unreinforced conditioned one while the latter is in an inhibitory phase (the phase of extinctive inhibition), acquires inhibitory properties through the contiguous relation. In other words, the additional stimulus, instead of becoming positively conditioned, becomes conditioned to the inhibition.

A number of experiments on conditioned inhibition in dogs are reported by Pavlov (4). Razran (5, 6) presents two excellent reviews in which, under the heading of "negative conditioning," a number of experiments on conditioned inhibition in animals and children are included. As space will not permit the reviewing of these studies in the present article the reader is referred to these authors.

The present experiment was designed to study the parallel development of positive conditioning and conditioned inhibition in humans. Thus, instead of being concerned with the development of conditioned inhibition after the establishment of positive conditioning[7] it was

[6]Hull (3) gives an extremely pertinent discussion of the biological significance of this mechanism.

[7]The procedure customarily employed by Pavlov (4).

concerned with the development of the two side by side. The object of the experiment, then, was (1) to determine whether conditioned inhibition could be set up in this way, and if so, (2) to study the course of its development.

A mechanical blow on the cheek just below the eye evoking an eyelid response was used as the adequate or conditioning stimulus, a faint buzz as the one to be conditioned, and a dim flash as the one to be rendered the conditioned inhibitor. The buzz was employed in two different combinations. In one of these it was given with no other stimulus preceding and was followed .37 seconds by the blow on the cheek; this will be referred to as the buzz-strike, or excitatory combination. In the other it was preceded .33 seconds by a flash and was always unreinforced; this will be referred to as the flash-buzz, or inhibitory combination.

Fifteen subjects were put through the experimental program which consisted of one period on each of two consecutive days. In each period the inhibitory and reinforcement combinations were presented practically an equal number of times in a pre-arranged "random" order. A 38.5-second interval between stimulations was always adhered to. At the end of each period tests of conditioning were made by sounding the buzz alone. The amount of differential or inhibitory conditioning was determined by a comparison of the magnitudes of the responses to the conditioned stimulus and inhibitory combination. The stimulations, in which R represents the reinforcement combination, I, the inhibitory combination, and C, the conditioned stimulus alone, were given in the following order:

First day

R. I, R, R, I, I, I, R, I, R, R, R, I, R, I, I, R, I, R, R, I, I, I, R, R, I*, R, I, I, R, *I, C, I, C, I,* R, R, I.

Second Day

I, R, R, I, I, I, R, I, R, R, I, I, R, R, R, I, R, I, I, R, I, R, R, I, I, R, R, I, R, *I, C, I, C, I.*

Figure 1 represents in units of 1/50 inch the average magnitude of the tracings produced by lid reactions at each stimulation during the two experimental periods. Figure 2 represents averages plotted from the same data from the respective thirds of the two periods. (The values here plotted are given in Table 1.)

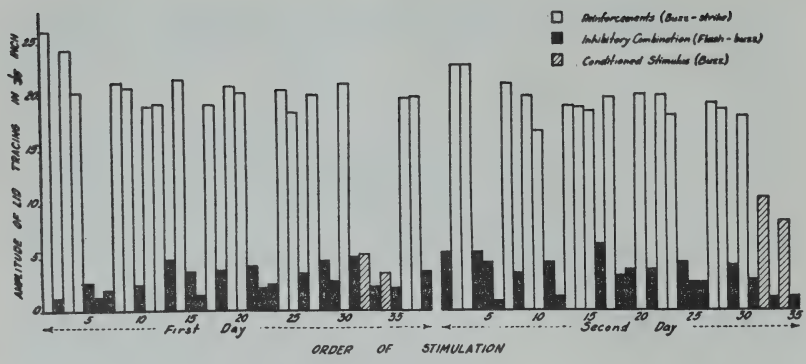

FIGURE 1

GRAPH SHOWING IN UNITS OF 1/50 INCH THE AVERAGE MAGNITUDE OF TRACING IN RESPONSE TO EACH SUCCESSIVE STIMULATION FOR THE TWO EXPERIMENTAL PERIODS FOR ALL FIFTEEN SUBJECTS

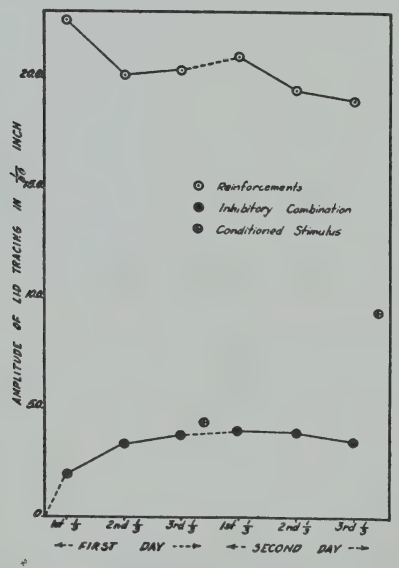

FIGURE 2

SHOWING THE AVERAGE MAGNITUDE OF TRACING FOR THE RESPECTIVE THIRDS OF BOTH DAYS IN RESPONSE TO (1) REINFORCEMENT (buzz-strike), AND (2) THE INHIBITORY COMBINATION (flash-buzz), AND THE AVERAGE MAGNITUDE OF TRACING IN RESPONSE TO THE CONDITIONED STIMULUS (buzz) AT THE END OF BOTH DAYS

TABLE 1

SHOWING FOR EACH 1/3* OF THE TWO PERIODS THE MEAN MAGNITUDE OF LID TRACING IN UNITS OF 1/50 INCH OF THE RESPONSES TO (1) REINFORCEMENT, AND (2) THE INHIBITORY COMBINATION; AND THE AVERAGE MAGNITUDE OF THE CONDITIONED RESPONSE AT THE END OF EACH PERIOD

	First period			Second period		
	1st 1/3	2nd 1/3	3rd 1/3	1st 1/3	2nd 1/3	3rd 1/3
Reinforcement	22.7	20.2	20.4	20.8	19.6	18.9
Inhibitory combination	2.0	3.43	3.8	4.0	3.9	3.5
Conditioned reaction			4.4			9.4

*The last two responses to reinforcement at the end of the first period are not included, so that each 1/3 represents 5 stimulations. As the values for the inhibitory combinations were computed from the first 16 of these combinations in both periods, the middle figure for each day is computed from 6 stimulations instead of 5.

It will be seen that the inhibitory combination, though never reinforced, gradually became somewhat excitatory in effect. As is shown in the lower curve of Figure 2, this excitatory tendency apparently reached its peak early in the second experimental period and then commenced to diminish gradually. Although the difference and direction between no two points on the curve can be considered significant from the standpoint of statistical reliability, the general continuity of the curve suggests that the combination develops excitatory properties fairly rapidly at first, but that these excitatory properties finally commence to diminish gradually.

It seems fairly evident that this acquisition of excitatory properties by the intended inhibitory combination is a true conditioning phenomenon. But there does remain the possibility of its being the result of a general lowered threshold of the reflex wink due to some factor such as eye-strain or fatigue. That this possibility, however, must be ruled out and that we are apparently dealing with a genuine conditioning phenomenon becomes evident from inspection of Figure 2. If this were an eye-strain or fatigue phenomenon one would expect it to show a rising curve on both days. As it shows a falling curve on the second day it seems reasonable to conclude that the excitatory tendency acquired by the intended inhibitory combination is a true conditioned phenomenon.[8]

[8]The acquisition of excitatory tendencies by the intended inhibitory combination suggested the possibility that positive conditioning of the second

Up to the present point the so-called inhibitory combination has been treated as if it were an excitatory conditioned stimulus, which,

TABLE 2

SHOWING IN UNITS OF 1/50 INCH THE AVERAGE MAGNITUDE OF LID TRACING IN RESPONSE TO THE INHIBITORY COMBINATION AND TO THE CONDITIONED STIMULUS DURING THE TEST SERIES AT THE END OF THE FIRST EXPERIMENTAL PERIOD

Subject No.	Combined winks to the three inhibitory combinations		Combined winks to conditioned stimulus
1	7.0		10.5
2	3.7		.0
3	13.7		26.5
4	.3		1.5
5	1.7		.0
6	9.7		1.0
7	.0		.0
8	.3		.5
9	.0		4.0
10	4.0		2.5
11	1.0		.5
12	4.0		8.0
13	1.7		5.5
14	.0		1.5
15	1.0		4.0
Mean	3.21		4.40
$P.E._M$	.68		1.16
r		.74	
Difference		1.19	
$P.E._D$		.80	
$\frac{D}{P.E._D}$		1.49	

order might be in the process of establishment at an early stage in the development of conditioned inhibition. To investigate this possibility, fourteen additional subjects were put through the experimental program of the first day up to the point indicated by the asterisk in the order of presentation. At this point, instead of being presented with the inhibitory combination, these subjects were given the flash alone. The flash, which had been tested previously to conditioning the buzz, had been found to produce no observable response on the record, but when presented alone at this point elicited positive responses in six out of the fourteen subjects. However, there is no assurance that this result is not attributable to a lowering of the threshold of the reflex wink to light as the result of eye-strain or fatigue. Therefore, it cannot be considered as a valid instance of second order conditioning.

at least up to a certain stage in its development, it undoubtedly is. But the additional stimulus, after the repeated pairing, does develop a very definite inhibitory effect upon the conditioned response when presented with the latter. This will now be brought out.

For the purpose of comparison with a view to detection of conditioned inhibition, averages of the responses to the three presentations of the inhibitory combination and of the two responses to the conditioned stimulus were computed for each test series. What is to be regarded as the test series is that constellation of stimuli which includes the unreinforced conditioned stimulus and appears at the end of each days. This is the italicized constellation *I, C, I, C, I.*

TABLE 3

SHOWING IN UNITS OF 1/50 INCH THE AVERAGE MAGNITUDE OF LID TRACING IN RESPONSE TO THE INHIBITORY COMBINATION AND TO THE CONDITIONED STIMULUS DURING THE TEST SERIES AT THE END OF THE SECOND EXPERIMENTAL PERIOD

Subject No.	Combined winks to the three inhibitory combinations		Combined winks to conditioned stimulus
1	.7		14.0
2	1.0		.0
3	.0		14.0
4	4.0		22.5
5	.3		19.5
6	6.7		16.0
7	.0		6.0
8	3		2.0
9	4.0		9.0
10	2.0		1.0
11	.0		.0
12	7.3		8.0
13	.0		11.5
14	.7		6.5
15	.0		11.5
Mean	1.8		9.43
$P.E._M$	.42		1.18
r		.24	
Difference		7.63	
$P.E._D$		1.15	
$\frac{D}{P.E._D}$		6.63	

Table 2 permits a comparison of the average magnitude of the conditioned responses with that of the responses to the inhibitory combination for the test series of the first day. The mean for the latter is 73% of that for the former. The significance of this difference, as attested by its critical ratio of 1.49 in terms of P.E., is practically nil. In the light of such an extremely slender difference coupled with an equally slender statistical reliability, no case can be justifiably made for the presence of conditioned inhibition at the end of the first period.

By the end of the second period, however, the difference between the two types of response is much more evident, as is shown in Table 3. At this point the magnitude of the mean response to the inhibitory combination is only 19 per cent of that to the conditioned stimulus. The difference between the two is over six times its probable error. Such a difference is well within the range of generally accepted reliability, and it is therefore reasonably safe to contend that by the end of the second period conditioned inhibition was well marked, though not complete.

The mean response to the inhibitory combination for the test series of the second day proved to be but 56% of that of the first day. The critical ratio of this difference, in terms of P.E., is 1.91, which indicates a likelihood of about one chance in ten that the direction of this difference is the result of fortuitous factors.

The mean conditioned response for the second day is 2.14 times that for the first. The actual difference between these means is 3.49 times its P.E., which indicates that there is approximately one chance in a hundred that the direction of the difference is the result of fortuitous factors.

Demonstration of the functioning of conditioned inhibition in this experiment depended upon the magnitude of the response to the conditioned stimulus being significantly greater than that of the response to the inhibitory combination. This requirement was satisfied at the end of the second experimental period.

Secondary Extinction

From the familiar type of experimental extinction, which is also designated primary extinction, Pavlov (4) differentiates what he calls *secondary extinction,* which is the weakening of other conditioned reflexes resulting from the extinction of the primary one. He says that this phenomenon of extinction involves

> . . . not only those reflexes which were based upon a common unconditioned reflex with the primarily extinguished one (homogeneous conditioned reflexes), but also those that were based upon a different unconditioned reflex (heterogeneous conditioned reflexes). Sometimes secondary extinction reaches a profound degree, involving even the unconditioned reflexes.

The present experiment was designed to determine whether secondary extinction is to be found in humans. It is concerned with homogeneous conditioned reflexes only.

The same flash and buzz employed in the preceding experiment were used here. Each of these was conditioned separately to the winking response by being accompanied by the blow on the cheek. The same approximately .35-second interval between the two components of a double stimulation and 38.5-second stimulus interval were maintained. The buzz was chosen as the conditioned stimulus to undergo active extinction, leaving the flash as the critical conditioned stimulus in which evidence of passive extinction was to be sought.

To determine the presence of passive extinction of the conditioned response to the flash, it was desirable to have samplings of its potency both prior to and immediately succeeding extinction of the response to the buzz. But, as the testing of a conditioned response necessarily renders it liable to some degree of experimental extinction, such a procedure could not be used. Instead, an indirect comparison had to be made.

As was brought out in the preceding experiment (Figure 2), by the end of the second day of reinforcement the magnitude of the conditioned response becomes greater than at the end of the first day. It seemed safe to assume, therefore, that if the flash were reinforced an equal number of times on both days, but its conditioned response when tested after extinction of the buzz on the second day were less than when tested at the end of the first day, passive extinction must have been operative. This difference between the magnitude of the response to the flash at the end of the first day and after extinction of the response to the buzz on the second day was therefore taken as the criterion of secondary extinction.

It seemed possible, at least, that should the buzz again be reinforced following its extinction an increase in the excitatory capacity of the flash might also occur. To ascertain this point the buzz was

again reinforced to restore its excitatory effect, after which the response to the flash was re-tested and compared with its state prior to this reinforcement and subsequent to extinction of the buzz. The difference in the status of the flash before and after reinforcement of the buzz was taken as an index of what may be called *secondary reinforcement.*

Ten subjects[9] constituting Group *A* were put through the program consisting of one experimental period on each of two successive days. During the establishment of conditioning the flash and buzz were reinforced more or less alternately according to a prearranged "random" order. The order of stimulation for this group, for the two days, in which (F) represents flash alone, (B), buzz alone, F, reinforced flash, and B, reinforced buzz, was as follows:

First day

(F), (B), F, B, F, B, B, B, B, F, F, B, F, F, F, F, B, F, B, B, B, F, F, F, F, F, B, B, B, B, B, (F), (F).

Second day

(F), (B), F, B, F, B, B, B, B, F, F, B, F, F, F, F, B, F, B, B, B, F, F, F, F, F, (B), (B), (B), (B), (F), B, B, B, B, (F), (F).

Analysis of the data from this group showed reinforcement of the buzz after its extinction to be accompanied by an increase in the excitatory capacity of the flash. It was therefore thought desirable to determine, if possible, whether this effect on the response to the flash was actually due to reinforcement of the response to the buzz, or merely to the lapse of time; that is, whether it was an instance of spontaneous recovery.[10] To clarify this question, eleven additional subjects were put through the same experimental procedure up to the testing of the flash after extinction of the buzz on the second day. Subsequent to this point, however, instead of being presented with four consecutive stimulations of the reinforced buzz, this group (which will be referred to as Group *B*) was simply allowed to wait the four stimulus intervals, after which tests of the flash alone were made. One subject failed to show any extinction during the five

[9]Those subjects who gave no conditioned reactions at the end of the first day were dropped from the experiment.

[10]Spontaneous recovery in dogs has been reported by Pavlov (4) and in humans by Switzer (10).

successive stimulations of the buzz alone. This subject was therefore eliminated from the group. The order of stimulation for this group for the two days, in which the symbols have the same significance as above and in which the additional symbol W represents no stimulation at the customary interval, was as follows:

First day

(F), (B), F, B, F, B, B, B, B, F, F, B, F, F, F, F, B, F, B, B, B, F, F, F, F, F, B, B, B, B, B, (F), (F).

Second day

(F), (B), F, B, F, B, B, B, B, F, F, B, F, F, F, F, B, F, B, B, B, F, F, F, F, F, (B), (B), (B), (B), (B), (F), W, W, W, W, (F), (F).

As Groups *A* and *B* were treated alike up through the series of five unreinforced presentations of the buzz on the second day, it was permissible to combine the data from the two in plotting the curve of experimental extinction. Figure 3 is a curve plotted from the average magnitude of tracing produced at each of the five successive presentations of the buzz alone for the twenty subjects of the com-

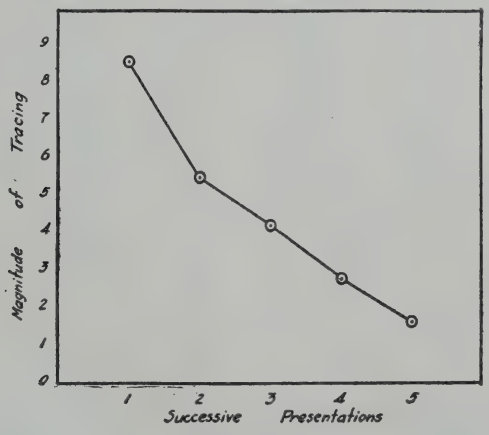

FIGURE 3

SHOWING IN UNITS OF 1/50 INCH THE AVERAGE MAGNITUDE OF TRACING IN RESPONSE TO EACH OF THE FIVE SUCCESSIVE UNREINFORCED PRESENTATIONS OF THE BUZZ FOR TWENTY SUBJECTS. (The data from one subject who showed no experimental extinction are not included.)

bined two groups. The actual values from which the curve is plotted and the reliability of the differences between successive points as expressed by their critical ratios are given in Table 4. It must be

TABLE 4

SHOWING IN UNITS OF 1/50 INCH THE MAGNITUDE OF THE LID TRACING IN RESPONSE TO EACH OF THE FIVE SUCCESSIVE PRESENTATIONS OF THE UNREINFORCED BUZZ*

Subject No.	Presentations				
	1st	2nd	3rd	4th	5th
1	5	5	2	13	1
2	9	3	0	0	0
3	2	3	2	0	0
4	7	3	0	0	0
5	4	0	1	0	3
6	11	10	3	7	0
7	13	5	7	0	0
8	16	3	4	2	4
9	21	23	19	6	0
10	1	0	0	0	0
11	9	5	3	3	2
12	14	14	9	13	6
13	2	1	0	0	0
14	15	2	5	0	0
15	16	4	3	3	2
16	3	17	15	2	7
17	1	0	0	0	0
18	7	0	0	0	0
19	9	9	8	3	6
20	8	3	4	4	3
Mean	8.65	5.50	4.25	2.80	1.70
$P.E._M$	.72	.83	.58	.36	.12
r	.48		.93	.34	.33
Difference	3.15		1.25	1.45	1.10
$P.E._D$	.99		.71	.89	.67
$\frac{D}{P.E._D}$	3.18		1.76	1.63	1.64

*The data from one subject who showed no experimental extinction are not included in this table.

borne in mind, however, that this can not be considered as a representative curve of the wearing down of a conditioned response resulting from lack of reinforcement, since the data from one subject

who showed no extinction are not included in the computations. This curve is given only to show the degree of extinction of the response to the buzz undergone in those subjects in whom a comparison of this extinction with the subsequent extinction of the response to the flash was to be made. In other words, it represents the extent of primary extinction with which the secondary extinction is to be compared.

Consideration will now be given to the effect of extinction of the response to the buzz on the magnitude of the response to the flash. Inspection of Table 5 computed from data from Group *A* enables

TABLE 5

SHOWING IN UNITS OF 1/50 INCH THE AVERAGE MAGNITUDE OF THE TRACING OF THE CONDITIONED LID RESPONSE TO THE FLASH AT THE END OF THE FIRST DAY, AFTER EXTINCTION OF THE RESPONSE TO THE BUZZ ON THE SECOND DAY AND AFTER REINFORCEMENT OF THE BUZZ ON THE SECOND DAY
(Group *A*)

Subject No.	Average of two responses to flash end of 1st day	Average of two responses to flash after extinction of response to buzz, 2nd day	Average of two responses to flash after reinforcement of response to buzz, 2nd day
1	1.0	1.5	2.0
2	7.5	2.0	10.5
3	2.0	.0	.0
4	2.5	.0	8.5
5	2.5	.0	1.5
6	9.0	.0	4.5
7	2.5	3.0	6.5
8	11.0	1.0	6.0
9	6.0	.5	18.5
10	6.5	.0	5.0
Mean	5.05	.80	6.30
$P.E._M$	.69	.21	1.08
r	—.07		.17
Difference	4.25		5.50
$P.E._D$	.73		1.06
$\frac{D}{P.E._D}$	5.82		5.19

a comparison of the mean magnitudes of response to the flash both before and after extinction of the conditioned response to the buzz. The former of these is 6.31 times as great as the latter. The actual

difference between the two is 5.82 times its probable error. This represents a fairly significant difference. Table 6, computed from

TABLE 6

SHOWING IN UNITS OF 1/50 INCH THE AVERAGE MAGNITUDE OF THE TRACING OF THE CONDITIONED LID RESPONSE TO THE FLASH AT THE END OF THE FIRST DAY, AFTER EXTINCTION OF THE RESPONSE TO THE BUZZ ON THE SECOND DAY, AND AGAIN AFTER A REST OF FOUR STIMULUS INTERVALS

(Group *B*)

Subject No.	Average of two responses to flash end of 1st day	Average of two responses to flash after extinction of response to buzz 2nd day	Average of two responses to flash after a rest of four stimulus intervals
1	3.0	3.0	.0
2	17.0	2.0	6.5
3	3.0	.0	.0
4	7.0	.0	.0
5	9.5	3.5	.0
6	13.5	1.0	7.0
7	8.0	.5	.0
8	3.5	.5	1.0
9	7.5	3.0	.0
10	3.0	1.0	1.0
Mean	7.50	1.45	1.55
$P.E._M$	.97	.27	.56
r	.21	—.03	
Difference	6.05	.10	
$P.E._D$	.95	.62	
$\frac{D}{P.E._D}$	6.37	.16	

data from Group *B,* makes possible a similar comparison. Here again the mean magnitude of response to the flash before extinction of that to the buzz is 5.17 times as great as after this extinction has taken place. The difference here is 6.37 times its probable error, which also indicates a fair degree of significance.

Subsequent to this extinction, the subjects in Group *A* were given four successive reinforcements of the buzz. These reinforcements of the buzz apparently produced more than a seven-fold average magnification of the conditioned response to the flash, as is shown in Table 5. This difference also seems to be of considerable significance as it is 5.19 times its probable error.

Thus far, however, it cannot be said with even a reasonable degree of assurance that this enhancement of the excitatory effect of the flash was due to secondary reinforcement, as the factor of spontaneous recovery has not as yet been taken into consideration. Group *B* was employed chiefly to test this point. By referring to Table 6 it will be seen that the magnitude of response to the flash has undergone no significant change as the result of the 193-second wait following its secondary extinction. The small increase shown is extremely unreliable, as evidenced by its critical ratio of .16. Table 7 makes

TABLE 7

SHOWING IN UNITS OF 1/50 INCH THE AVERAGE MAGNITUDE OF THE TRACING OF THE CONDITIONED LID RESPONSE TO THE FLASH AFTER REINFORCEMENT OF THE BUZZ AT THE END OF THE SECOND DAY (Group *A*), AND THAT AFTER THE 192.5-SECOND REST AT THE END OF THE SECOND DAY (Group *B*)

	Group A		Group B
	2.0		.0
	10.5		6.5
	.0		.0
	8.5		.0
	1.5		.0
	4.5		7.0
	6.5		.0
	6.0		1.0
	18.5		.0
	5.0		1.0
Mean	6.30		1.55
$P.E._M$	1.08		.56
Difference		4.75	
$P.E._D$		1.22	
$\frac{D}{P.E._D}$		3.89	

possible a comparison of the conditioned responses to the flash in Group *A* after reinforcement of the previously extinguished buzz, with those of Group *B* at the corresponding point, but with a mere lapse of time replacing the reinforcement of the buzz. There being only this one difference in procedure the two groups may be considered comparable. The experimental difference shown here is again fairly reliable, being 3.89 times its probable error. This is less than the generally acceptable standard of 4.0, but only slightly so. If the reliability of this difference is acceptable the indication is that sec-

ondary reinforcement must have taken place over and above any spontaneous recovery which may also have occurred.

These results indicate that in humans the extinction of a conditioned response for a particular stimulus from one receptor may have a very marked effect on the strength of homogeneous conditioned responses from other receptor organs. They stand in essential agreement with Pavlov's previous findings in dogs. The present results indicate, however, that the effect on other conditioned responses is not only operative in extinction but in reinforcement as well. Since the conditioned response may be considered a learned activity, both secondary extinction and reinforcement may be regarded as instances of transfer of training. In the present experiment the degree of this transfer was marked.

Summary

Experiments were designed to study in humans two of the inhibitory phenomena encountered by Pavlov in conditioned reflex studies in dogs. These phenomena were (1) *conditioned inhibition* and (2) *secondary extinction.* The eyelid reaction evoked by a mechanical blow on the cheek was the basic response used.

(1) *Conditioned Inhibition.* The course of development of a differential response resulting from the alternate presentation of a previously neutral stimulus (buzz) in two different combinations was traced. In one combination, (*A*), it was accompanied only by another previously neutral stimulus (flash); in the other combination, (*B*), it was accompanied by the conditioning stimulus (blow on the cheek). Averages from 15 subjects indicated that combination *A* became excitatory, reached a peak of excitation, and then gradually became less excitatory. The buzz, as a result of its participation in combination *B,* was found to have increased in excitatory capacity throughout the experiment. By the end of the second experimental period the response to the buzz when presented alone was found to be significantly greater than when accompanied by the extra stimulus. The inference is that combination *A* developed excitatory and inhibitory properties side by side; in the early stages excitation was dominant, in the later, inhibition. In so far as inhibition was developed the extra stimulus must be considered a *conditioned inhibitor.*

(2) *Secondary Extinction.* Two previously neutral stimuli (buzz and flash) were alternately presented with a conditioning stimulus

(blow on the cheek) until both had been rendered conditioned stimuli. The conditioned response to the buzz was then subjected to *experimental extinction* by being presented repeatedly without reinforcement. In the group of 20 subjects employed this procedure resulted in a corresponding diminution of the conditioned response to the flash. Subsequent reinforcement of the buzz in 10 of the subjects produced a corresponding augmentation of the response to the flash. A control, effected by allowing the remaining 10 subjects to wait an interval of time equal to that consumed by the subsequent reinforcement in the other 10, indicated that this augmentation of the response did not result from the mere passage of time (spontaneous recovery). The operation of both *secondary extinction* and *secondary reinforcement* seems to have been demonstrated in the experiment.

The results corroborate those of Pavlov and point toward an interrelation between conditioned reactions.

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ÉTUDES DES INHIBITIONS DANS LES RÉPONSES CONDITIONNELLES

(Résumé)

On a fait des études de l'*Inhibition Conditoinnelle* et de l'*Extinction Secondaire* de la réaction conditionnelle de la paupière chez des humains.

L'Inhibition Conditionnelle. On a présenté un bourdonnement lèger en l'ordre alterné au hasard en deux combinaisons différentes. Dans l'une on l'a fait précéder du tiers d'une seconde par un jet léger de lumière; dans l'autre on l'a fait suivre du tiers d'une seconde par un coup sur la joue. Après environ 30 présentations de chaque combinaison les 15 sujets employés ont fait d'une façon signifiante un plus petit clin d'oeil au bourdonnement précédé du jet de lumière que celui fait au bourdonnement seul. Le jet de lumière a dû devenir un *inhibiteur conditionnel.*

L'Extinction Secondaire. On a présenté alternativement un bourdonnement léger et un jet de lumière avec un coup sur la joue jusqu'à ce que les deux soient devenus des stimuli conditionnels pour la réaction de la paupière. Alors on a fait subir l'extinction expérimentale au bourdonnement. Ce procédé a eu comme résultat une diminution correspondante de la réponse conditionnelle au jet de lumière chez les 20 sujets employés. Le renforcement subséquent du bourdonnement chez 10 sujets a produit une augmentation correspondante de la réponse au jet de lumière, laquelle on a montré ne pas être due au rétablissement spontané. Il paraît que l'on a démontré et l'*extinction secondaire* et le *renforcement secondaire* dans l'expérience.

Les résultats corroborent ceux de Pavlov et indiquent une inter-relation entre les réactions conditionnelles.

SHIPLEY

UNTERSUCHUNGEN ÜBER HEMMUNGEN BEI BEDINGTEN AKTIONEN

(Referat)

Untersuchungen der bedingten Hemmung und sekundären Ausschaltung der bedingten Augenlidreaktionen wurden bei Menschen ausgeführt.

Bedingte Hemmung. Ein schwaches Summen (buzz) wurde in einer zufälligen, abwechselnden Reihenfolge und in zwei verschiedenen Kombinationen dargeboten. In der einen Kombination ging ein schwacher Lichtstrahl um 1/3 Sekunde voran; in der anderen wurde es von einem Schlag an die Wange um 1/3 Sekunde gefolgt. Nach einigen 30 Darbietungen jeder Kombination zeigten 15 Vpn. ein bedeutend kleineres Blinken auf das Summen, wenn der Strahl voranging, als wenn es allein dargeboten wurde. Der Strahl muss eine *bedingte Hemmung* geworden sein.

Sekundäre Ausschaltung. Ein schwaches Summen und ein Lichtstrahl wurden abwechselnd mit einem Schlag an die Wange dargeboten, bis beide bednigte Reize für die Augenlidreaktion geworden waren. Das Summen wurde dann einer experimentellen Ausschaltung ausgesetzt. Dieses Verfahren hatte eine entsprechende Abnahme der bedingten Aktion auf den Strahl bei 20 Vpn. zur Folge. Nachherige Verstärkung des Summens bei 10 Vpn. erzeugte eine entsprechende Zunahme der Reaktion auf den Strahl, die als nicht die Folge einer spotanen Wiederherstellung nachgewiesen wurde. Sowohl die sekundäre Ausschaltung als auch die sekundäre Verstärkung scheinen durch dieses Experiment nachgewiesen worden zu sein.

Die Resultate bestätigen die von Pavlov und weisen auf eine Zwischenbeziehung zwischen bedingten Aktionen.

SHIPLEY

A STUDY OF HYPNOTICALLY INDUCED COMPLEXES BY MEANS OF THE LURIA TECHNIQUE*

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INTRODUCTION

Recently Luria (3, 2, 4, 5) has experimented with a technique which may be applied to the investigation of affective conflicts. This method[2] involves the association of higher central nervous system processes with a voluntary movement so that conflicts in the former are disclosed in the latter. Experimentally the central processes are activated by the verbal stimuli of an association test, the subject being instructed to make a slight pressure with his preferred hand on a tambour simultaneously with every verbal response. If the verbal stimuli do not arouse affective conflicts the voluntary pressures are regular in character, but if a conflict is aroused the pressure curves become irregular. Luria explains this effect as follows: Having trained a subject to associate a motor response of the preferred hand with every verbal response, thereby establishing a close functional relationship between them,[3] any word occurring to the subject which he does not give as a response will appear in the voluntary movement as a partial reaction. It is assumed here that the inhibition of the verbal response is associated with affect, i.e., the subject does not respond with the first word

*Accepted for publication by Carl Murchison of the Editorial Board and received in the Editorial Office, January 17, 1934.

[1]The division of labor in this study was as follows: the problem was set and the data analyzed by the first two authors (psychologists), the hypnotic work and organization of the complexes done by the third author (a psychiatrist), and the experimental work performed by the first author.

[2]In general we shall adhere to Luria's terminology in this paper.

[3]Luria argues that previous work on affection and emotion has yielded disappointing results because there was no intimate relationship between the affect and such physiological expressions as blood pressure, heart rate, respiration, etc. According to Luria, the expressiveness of any motor system will depend upon its degree of inclusion in the psychological structure where the conflict is located, hence the preferred hand is selected to make the voluntary pressures because of the close relationship between the speech centers and the neural control of the preferred hand.

since some complex would be revealed. Also the pressure curve may lose its smooth regular character or, to follow the Luria terminology more closely, the normal, voluntary movement is discoordinated or disorganized because stimuli which elicit responses possessing affect may also arouse larger amounts of excitation than stimuli eliciting non-affective responses. This excitation tends to discharge itself immediately via the voluntary motor pathway. Luria has referred to this tendency as the "law of the catalytic action of the stimulus." This law appears to be a corollary of another, the "law of the decreased action of the functional barrier." The functional barrier is a cortical property. It regulates by inhibition the motor activities of the organism, giving them an integrated character. Affective excitation weakens the functional barrier and hence the motor activities become disorganized. A third law is that of the "mobilization of inadequate masses of excitation." This seems to involve "neurodynamical perseveration." The excitation which accompanies the affect is not always discharged completely via the verbal response, hence some movements will persist in the preferred hand after the voluntary response. Under conditions of large amounts of excitation a further spread to other motor systems may occur, disturbing, for example, respiration and/or causing involuntary movements of the non-preferred hand.[4]

It is not our purpose to review the numerous experiments reported by Luria upon which these so-called laws are based. We were interested primarily in the possibilities of the Luria technique for obtaining information about the affective conflicts of a subject and its possible application to psychotic patients. For this purpose we repeated, as part of an exploratory procedure, one of his important experiments, that of the attempted induction of a conflict in a subject by means of hypnosis. Such a procedure affords the opportunity of examining a subject before, during, and after the establishment of a conflict.

To produce a conflict in a subject Luria fabricated a story of a reproachable act committed by the subject—an act which would be contrary to the subject's usual personality trends. A number

[4]This exposition is our attempt to state the Luria theory succinctly. Other attempts may be found in recent reviews of Luria's latest publication (4): Brown, J. F., *Psychol. Bull.* 1933, **30**, 376-381; Kubie, L. S., *Psychoanal. Quar.* 1933, **2**, 330-336; Geldard, F. A., *J. Gen. Psychol.* 1933, **9**, 485-487; Taylor, W. S., *J. Abn. & Soc. Psychol.*, 1934 (to be published).

of critical words were taken from this story and placed in a list of control words which were not specific to the story. The total list was presented in the setting of a free discrete association procedure. The subject was required to press with his preferred hand on a tambour with each verbal response. He was then hypnotized, and the story recounted to him. After this he was awakened and the combined word-association and motor-response method was repeated. Under hypnosis the conflict was removed. This was followed by a waking control session. If the theory and technique are valid the critical words should show discoordinated voluntary pressure curves as compared with those of the control words. This assumes, of course, that the suggested story was accepted by the subject, a conflict produced in him, and that it had been removed successfully. In addition to the voluntary pressure curves, Luria recorded verbal reaction-times, and in some cases involuntary movements from the non-preferred hand and respiration. All were recorded on an ordinary kymograph.

Apparatus, Technique, and Population

This was the experiment[5] which we repeated with some modifications. Luria's list usually contained 20 to 30 words with 6 to 9 "critical" words. We used 100 words, including 10 taken from the fictitious story, to avoid, if possible, perseveration effects and to give more control material. Furthermore, the "critical" words were separated by 7 to 10 control words whereas Luria often placed two or three "critical" words together. In addition, hypnotic control experiments were introduced and in some cases the control and the "complex" sessions were repeated to study the effects of hypnosis and of repetition *per se*. By way of definition, the term "complex" is used as referring to the story of a reproachable act committed by the subject intended to produce an affective disturbance or conflict in him.

Four male and eight female subjects between the ages of 20 to 30 years were used. This group consisted of four medical internes, two graduate students in psychology, two nurses, two occupational therapists, and two college graduates doing special work about the hospital. All were well-trained hypnotic subjects and fairly well known to us. None of them had any knowledge of the

[5]The work was done during the years 1930 to 1932.

Luria theory or technique. We shall present in detail the complex, the experimental procedure, and the results on one subject. The results obtained on the other subjects will then be summarized.

Detailed Report on One Subject

The complex for the sample subject, a male aged 24, was narrated as an account of his personal experience and an attempt was made to establish it as a falsification of memory. The story in summary form was as follows: One night, while visiting some friends, he met a girl to whom he was much attracted. During the conversation, attention was called to her new brown silk dress and she explained that, although not able to afford it, she had bought the dress hoping to make a good appearance when applying for employment. He gave her a cigarette and lighted one also. While smoking, he noticed the smell of burning cloth occasioned by contact of his cigarette with the girl's dress. Unobtrusively he withdrew his hand, noting with relief that the girl had not yet noticed the accident and that she held her own cigarette above the burned hole. The girl soon became aware of the damage. She attributed it, however, to a spark from her own cigarette. He tried to take the blame by assuming the responsibility of having given her the cigarette but the girl refused his apparent generosity. The next day, by which time he had summoned up enough courage to tell her the truth in order to save his self-respect, he found that she had left the city.

From this account ten words: Silk, Dress, Brown, Cigarette, Burned, Hole, Blame, Damage, Smell, Self-respect, were selected as "critical" words and placed as Nos. 7, 16, 28, 40, 49, 60, 68, 77, 88, and 98 in the list of words[6] (see Table 1).

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[6]The ninety other words consisted of sixty-six from the Kent-Rosanoff Association Test (1) which give high frequencies of "most common" responses and which might be considered as neutral in character, i.e., generally without emotional tone; five which we believed significant for schizophrenic patients—words 10, 31, 46, 65, and 85; five significant for psychotics in general—words 6, 13, 34, 58, and 71; and five words which are often of affective value for normal individuals—words 19, 37, 55, 74, and 91. (Words 37 and 91 appear also in the Kent-Rosanoff list.) The remaining eleven words are "double-barreled," that is, they may easily be taken in more than one sense. These were Nos. 2, 4, 22, 43, 62, 63, 80, 90, 93, 95, and 100. (Words 25, 75, and 82 among the Kent-Rosanoff group may also be considered as "double-barreled.") Since our ultimate purpose was the application of the procedure to psychotic patients, we introduced the affective and "double-barreled" words, planning to use the responses from the normal subjects as control material.

In the experimental room the subject reclined on a *chaise longue* and rested his finger tips on deep, large tambours, one on each side, the forearms being supported by the wide arms of the *chaise longue*.[7] He was instructed to respond to the verbal stimuli of the association test with the first word which came to him and simultaneously with his response to make a downward pressure on the tambour with his preferred hand. Voluntary responses of the preferred hand and such involuntary movements of the non-preferred hand as might occur, as well as thoracic respiration and verbal reaction-time, were recorded on a special long-paper kymograph. The experimenter, seated out of the subject's view, gave the verbal stimuli, wrote down the verbal responses, and marked the verbal reaction-time with a telegraph key. A practice series of 20 words, none of which appeared in the experimental list, was first administered in order to establish the association of verbal response with simultaneous voluntary movement of the preferred hand. The list of 100 words was then given in a *waking control session*. After this the subject was hypnotized and the procedure repeated. This was the *hypnotic control session*. At the next experimental period—usually the following day—the subject was hypnotized first, the complex story told to him, and the experiment performed during hypnosis. This constituted a *hypnotic complex session*. The subject was then awakened from the trance and a *waking complex session* held. After this the subject was re-hypnotized and an attempt made to remove the conflict by giving him insight into the situation and permitting him to understand the falsity of the story. At a third period, additional *hypnotic* and *waking control sessions* were held.

In this particular case two hypnotic controls instead of one were obtained after the complex was removed.[8] [9] Also the complex was

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[7]Our tambour system, while not quite like that of Luria, is not different in its essential requirements.

[8]All the hypnotic work was done by the psychiatrist. Hypnotic rapport with the subject for experimental procedures was transferred to the experimenter, the same instructions being used on all subjects. The psychiatrist kept all subjects under observation for at least a month to note any possible residual effects of the experiment. (None were observed.) Close supervision was particularly maintained in those cases in which the subject was allowed to keep the complex overnight. All trances were of a profound somnabulistic type, characterized by dissociation and, with one exception which will be discussed later, apparently by total amnesia for trance events. Administration of the complex was achieved by two sets of instructions identical for all subjects. These were planned to pre-

not removed in this subject for 24 hours. That night he slept poorly, awakened with a headache which persisted until the removal of the complex in the afternoon, had no appetite, was resentful and antagonistic toward the hypnotist, and somewhat uncooperative towards additional hypnosis. He was unable to assign any reason for these manifestations. Throughout the day he gave away his cigarettes and apparently could not enjoy smoking. He rationalized this behavior by the statement that he "guessed" he was giving up the habit. We offer this as evidence that the attempt to induce the conflict produced a profound reaction in the subject.

The results have been analyzed within each session and from session to session. Various aspects of change in the verbal, voluntary, involuntary, and respiratory responses, in reaction-time and certain other aspects of behavior such as bodily movement, laughing, sighing, etc., were considered for different word classes. The word classes were the following:

1. *"Complex"* words—the ten words taken from the story told to the subject.

2. *"Complex-Associated—First Type."* These were words which the subject himself apparently connected in some way with the story, as indicated by the verbal responses. For example, in the subject under discussion, the stimulus word "Smooth" (No. 18) elicited the response "rough" in the control sessions and in the waking complex session, but in the hypnotic complex session the response was "silk." To qualify for classification as a "Complex-Associated —First Type" word the response had to appear to all three of the authors as definitely related to the complex situation, to be one of the words actually used in the complex story, and not to have appeared in any control session prior to the induction of the complex. This criterion for selecting "Complex-Associated—First

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pare them to "recall" the complex as an actual memory and to strengthen their acceptance of this "memory" and their emotional reaction to it. Removal of the complex was accomplished by the hypnotist's reviewing the preliminary instructions, the story, and the final instructions, indicating the falsity of the whole account, and allowing the subject to verify in his own mind the unreality of the entire complex story. This was—had to be—done in both the trance and waking states.

[9]Each session took about fifteen minutes. The stimulus words were given as rapidly as the experimenter could write down the verbal responses, except when a disturbance appeared. In the latter case the disturbance was allowed to subside before the next stimulus was given.

Type" words we consider as being conservative and as likely to result in the omission of some items since the same response in control and complex sessions may have a different meaning for the subject. This point will be discussed later.

3. *"Natural Complex"* words. These were chosen on the basis of our knowledge of the person and from what he reported when the list was reviewed with him after the conclusion of the experiment. In this particular person they were words which presumably would usually arouse some affect outside of the experimental situation. The stimulus word "Fall" (No. 90) was such a one. The subject two years previously had been in an aeroplane accident, in which he had broken an ankle.

4. Reference to Table 1 shows that the same word (No. 90) elicited a response in the first control session which might be connected with such a "natural complex," but after the complex induction the responses changed to "light" and "spark," which fact led us to believe that this stimulus word also became related to the complex. Because there were a number of such words which changed in class from one session to another, an additional class was formed, called *"Natural Complex+Complex-Associated"* words.

5. A *"Complex-Associated—Second Type"* classification was made on the basis of disturbed non-verbal responses which might be related to the complex story in an indirect way. We made the assumption here that the technique used *did* reveal the presence of affect and attempted to see if we could establish some association with the complex story. Sometimes the subject could explain why he responded with the particular word. In either case the stimulus was put into the second type of *"Complex-Associated"* words. Such information was obtained from the subject after the experiment was finished and the procedure had been explained to him. This was done by examining with him each of the verbal responses.[10] Obviously there are many more possible sources of error in this classi-

[10]This was done some time after the conclusion of the experiments. In the present case there were six "Complex-Associated" words of the second type. The stimulus word "Dark" (No. 9) elicited the response "room" in the hypnotic complex session. This was so classified because the subject informed us that he had placed the complex situation in a poorly lighted room to make the action more plausible. The response "beautiful" to "Fairy" (No. 63) was thought by us to have come about through "girl"—i.e., "Fairy"→"girl"→"beautiful." (The attractiveness of the girl had been emphasized in the complex story.) "Guilty" (No. 58) yielded two

fication than in the others and the results therefore must be scrutinized with great care.

6. All the remaining words were called *"Neutral."* Our knowledge of each subject's life was not adequate and the "Natural Complex" class probably suffers mostly on the side of omission. It is also likely that among the "Neutral" words are some which should have been classed "Natural Complex" and "Complex-Associated."[11]

Each of these word classes was first analyzed for the number of "disturbances" in the verbal and non-verbal material from session to session. By a "disturbance" in the verbal response is meant any significant word from the complex story which first appears in a complex session, e.g., to the *C* word "Brown" (No. 28) the subject's responses for the first control sessions are "eyes" and "color," whereas in the complex sessions the responses are "burn" and "silk"; after the removal of the complex the responses are "white," "color," and "white." Here the responses "burn" and "silk" are rated as verbal disturbances. Because of the criteria set, it is likely that some verbal disturbances may have been omitted, e.g., the response "clothes" to "Dress" (No. 16) in Session IV. Since "clothes" appeared in the second control we did not count the same response in this session as a verbal disturbance. It may have been specific to the complex here and have had an entirely different significance for the subject. In the voluntary, involuntary, and respiratory responses any fairly definite deviation from the normal was counted as a disturbance, after agreement by two of the authors working independently and then combining judgments. If there was disagreement as to the presence of disturbances the non-verbal response

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unusual reactions in the complex sessions, "self-conscious," and "no." These responses might be related to the emphasis on the loss of self-respect in the complex. "Persecute" (No. 71) as stimulus, with the unusual response "me" may fall into the same context. The subject reported that the response "necklace" to "Neck" (No. 80) resulted from his visualizing the girl in the story as wearing a necklace. The responses "stores" and "display" to the stimulus "Street" (No. 96) may have been connected with window displays of new dresses, since the fact that the girl was wearing a new dress was stressed in the story.

[11]Hereafter we shall refer to the word classes by the following symbols:
C—Complex
CA_1—Complex-Associated—First Type
CA_2—Complex-Associated—Second Type
NC—Natural Complex
CA_1+NC—Complex-Associated—First Type+Natural Complex
N—Neutral

was considered as not disturbed. In the voluntary responses irregularities in the base line after the stimulus word was given, or in the pressure response, or after the stylus had returned to the base line but before the next stimulus word was given, were recorded as voluntary disturbances. Figure 1 gives examples of these.

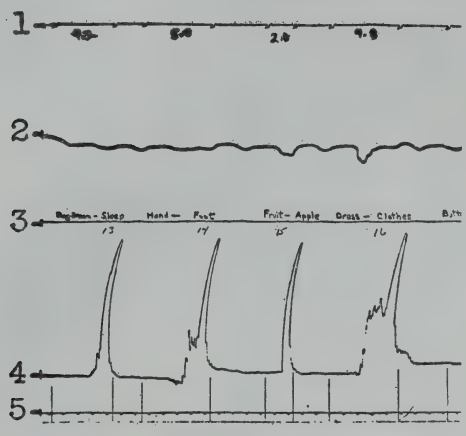

FIGURE 1

This figure is from the waking complex Session IV of the sample subject (No. 1). *1.* Verbal reaction-time. *2.* Respiration. *3.* Left hand, involuntary. *4.* Right hand, voluntary. *5.* Time in seconds. The voluntary pressures on words Nos. 14 and 16 are disturbed. The respiration line does not show characteristic breathing since the pneumograph was not drawn as tightly about the chest as was the usual practice. The loose adjustment was made in this subject because such large inspirations generally accompanied his respiratory disturbances that the stylus of the recording tambour moved off the kymograph.

Involuntary changes in the non-preferred hand consisted of either an increase in tremor amplitude or shifts in the base line. Respiratory changes were those which involved sudden inspirations or expirations, or increased depth or rate of breathing. The respiration curve was complicated by the chest movements which accompanied the verbal response, hence these had to be considered when rating respiratory disturbances. A verbal reaction-time was considered as disturbed only if it was extremely long.[12] [13]

[12]An attempt was made to determine which reaction-times might be considered disturbed. This was first done by distributing the reaction-times of every session of each subject and computing the S.D. However, because of the illegitimacy of such computations on a heterogeneous population and because of the skewed distributions which we obtained in many cases—

Stimulus	Class	Session No. I Waking Control 3-8-32 3:45 P.M. Response	Disturbances: Verbal	Voluntary	Involuntary	Respiration	Delayed R.T.	Total	Individual Reaction Times	Session No. II Hypnotic Control 3-8-32 4:05 P.M. Response	Disturbances: Verbal	Voluntary	Involuntary	Respiration	Delayed R.T.	Total	Individual Reaction Times	Session No. III Hypnotic Cosplex 3-9-32 4:30 P.M. Response	Disturbances: Verbal	Voluntary	Involuntary	Respiration	Delayed R.T.	Total	Individual Reaction Times
1. Table	M	1. chair							1.4	1. chair							1.5	1. chair							1.5
2. Box	M	2. wood							1.5	2. square				*		1	2.7	2. square							2.6
3. Music	M	3. sheet							1.9	3. sheet							1.9	3. piano							2.8
4. Ball	M	4. bat							2.1	4. bat							1.8	4. bat							2.0
5. Man	M	5. woman							2.0	5. woman							1.8	5. woman							2.2
6. Queer	N	6. funny							2.6	6. funny							2.0	6. extraordinary							3.0
7. Silk	C	7. wool							1.9	7. wool							1.9	7. dress	*						2.0	1
8. Eating	N	8. food							2.2	8. food							1.7	8. table			*				2.6	
9. Dark	CAg	9. light							1.9	9. light							1.8	9. room							2.8	1
10. Important	N	10. necessary							2.2	10. pertinent				*	*	2	5.2	10. pertinent							3.3	
11. Black	N	11. white							1.5	11. white							1.6	11. white							2.8	
12. Soft	CA1	12. hard							1.8	12. hard							2.0	12. hand		*					2.3	1
13. Day-dream	NC	13. dream							2.5	13. stupor							3.4	13. sleep							2.9	
14. Hand	CA1	14. foot							1.9	14. foot							1.8	14. girl		*					2.3	1
15. Fruit	N	15. apple							1.9	15. apple							2.5	15. apple							2.3	
16. Dress	C	16. undress							2.2	16. clothes							2.2	16. burn		*					2.8	1
17. Butterfly	MC	17. insect							1.9	17. insect							1.9	17. insect							2.9	
18. Smooth	CA1	18. rough							1.6	18. rough							2.1	18. silk		*					2.5	1
19. Kiss	NC	19. osculate	*						4.9	1 19. osculate							2.9	19. pretty							3.8	
20. Chair	N	20. table							1.8	20. table							2.0	20. table							2.3	
21. Sweet	CA1	21. sour							1.9	21. taste							2.9	21. girl	*						3.0	1
22. Push	N	22. pull							2.0	22. pull							1.9	22. pull							2.2	
23. Woman	CA1+NC	23. man							1.7	23. female							2.5	23. dress	*						4.2	1
24. Comfort	N	24. sleep							2.3	24. bed							2.2	24. bed							2.9	
25. Yellow	N	25. blue							1.8	25. color							1.9	25. color							2.2	
26. Wish	N	26. hope							2.0	26. hope							1.8	26. hope							2.5	
27. River	N	27. deep							2.4	27. outlet		*					3.4	1 27. water							2.8	
28. Brown	C	28. eyes							2.5	28. color							1.6	28. burn	*						3.5	1
29. Beautiful	CA1	29. nature							2.2	29. mother							2.9	29. pretty	*						3.5	1
30. Window	N	30. house							2.3	30. light							3.1	30. room							2.7	
31. Electricity	N	31. motor							2.7	31. generator							3.1	31. light							2.2	
32. Citizen	NC	32. city	*		*			2	4.2	32. population							3.7	32. man							2.5	
33. Foot	N	33. hand							1.8	33. hand							1.9	33. shoe							2.4	
34. Voices	N	34. hallucins.							3.1	34. chords							3.0	34. hallucination							3.2	
35. Needle	N	35. thread							1.9	35. thread							2.6	35. thread							3.2	
36. Short	N	36. long							1.9	36. long							2.1	36. long		*					3.2	1
37. Stomach	N	37. food							2.6	37. intestines							2.4	37. food		*					3.2	1
38. Whistle	N	38. blow							1.9	38. noise							2.9	38. sigh							2.8	
39. Slow	N	39. fast							1.7	39. swift							2.3	39. swift							2.1	
40. Cigarette	C	40. smoke							1.7	40. tobacco							2.5	40. burn	*						2.7	1
41. High	NC	41. low							1.6	41. skyscraper				*	*		4.0	2 41. low							2.9	
42. Working	N	42. writing			*				3.3	1 42. labor							2.1	42. thinking							2.5	
43. Monkey	N	43. tail			*				5.8	2 43. animal							2.6	43. animal							2.6	
44. White	CA1	44. black							4.3	44. color							2.1	44. black							2.9	
45. Rough	CA1	45. smooth							1.8	45. smooth							1.7	45. hole	*	*					4.4	2
46. Mission	N	46. errand							4.6	46. errand							3.2	46. errand							3.3	
47. Cabbage	N	47. turnips	*						3.9	1 47. vegetable							2.5	47. vegetable							3.3	
48. Hard	CA1	48. soft							2.0	48. soft							1.8	48. regret	*	*		*	*		5.1	4
49. Burned	C	49. black							2.9	49. chemistry				*	*	2	5.7	2 49. dress	*						2.8	1
50. Sleep	N	50. dream							2.0	50. dream							2.3	50. slumber							3.5	
51. Deep	N	51. river							2.5	51. river							2.2	51. river							3.7	
52. Girl	CA1+NC	52. boy					*	1	5.6	1 52. female							2.0	52. dress	*						3.5	1
53. Sour	N	53. sweet					*	1	5.1	1 53. acid							4.2	53. sweet							3.0	
54. Soldier	N	54. war							1.9	54. fight		*					2.8	1 54. brave							3.5	
55. Marriage	NC	55. house	*		*			2	4.6	2 55. woman							2.0	55. live							4.4	
56. Eagle	N	56. bird							4.3	56. bird							2.1	56. bird							2.5	
57. Boy	N	57. girl							1.9	57. child							2.8	57. girl							2.2	
58. Guilty	CAg	58. insect							2.4	58. crime					*	1	4.2	1 58. self-conse.							2.9	
59. Health	N	59. doctor							2.2	59. doctor							3.1	59. sanitarium		*					2.7	1
60. Hole	C	60. well					*		6.7	1 60. well							3.3	60. burn	*						2.9	1
61. Memory	CA1	61. thought							2.4	61. brain							2.5	61. dread	*						3.2	1
62. Pool	N	62. swim							3.9	62. water							2.7	62. swim							2.8	
63. Fairy	CAg	63. unreal							4.1	63. book							3.2	63. beautiful							3.5	
64. Cottage	N	64. yard							2.7	64. shelter							3.0	64. house							3.0	
65. Concentrate	N	65. think							2.0	65. think							2.5	65. meditate		*			*		4.8	2
66. Lamp	CA1	66. light							2.9	66. light							3.2	66. burn	*						2.7	1
67. Hungry	N	67. food							2.0	67. stomach	*						3.3	1 67. sick		*					3.2	
68. Blame	C	68. accuse			*				4.1	1 68. accuse							2.1	68. me	*						2.6	1
69. Ocean	N	69. sea							3.0	69. sea							1.9	69. deep							3.1	
70. Light	N	70. dark							2.6	70. dark							2.7	70. lamp							3.6	
71. Persecute	CAg	71. elec.chair	*				*	2	5.5	71. punish							3.9	71. me							3.5	
72. Long	N	72. short							1.6	72. short							1.8	72. short							2.9	
73. Swift	N	73. slow							1.8	73. slow							2.4	73. quick							2.8	
74. Sex	CA1	74. male		*					3.8	1 74. male & fem.							2.9	74. girl	*						2.8	1
75. Square	N	75. round							1.8	75. round							2.5	75. round							2.8	
76. Bitter	CA1	76. sweet							1.7	76. taste							2.4	76. taste							3.1	
77. Damage	C	77. ruin							2.2	77. ruin							2.9	77. ruin							2.0	
78. Hammer	N	78. nail							1.4	78. nail							1.7	78. nail							2.7	
79. City	N	79. street							2.0	79. street							1.9	79. street							2.9	
80. Neck	CAg	80. ear							3.3	80. hurt							2.9	80. necklace							3.1	
81. Butter	N	81. bread							2.1	81. milk							2.7	81. milk							2.5	
82. Blue	N	82. sky					*	1	1.7	1 82. color							1.8	82. sky							2.6	
83. Heavy	N	83. light							2.4	83. light							2.2	83. light							2.3	
84. Salt	N	84. sugar	*						1.9	1 84. table							2.8	84. table							3.1	
85. Unworthy	CA1	85. worthless							3.2	85. useless							3.1	85. self-respect	*	*			*		5.0	3
86. Joy	CA1	86. sorrow							2.2	86. sorrow		*					3.0	1 86. sorrow							3.2	
87. Bed	N	87. sleep							2.3	87. sleep							2.4	87. sleep							2.5	
88. Smell	C	88. taste							2.0	88. olfactory							3.7	88. burn	*						2.9	1
89. Tobacco	CA1	89. smoke							1.8	89. leaves							2.9	89. dean	*						2.0	1
90. Fall	CA1+NC	90. hurt					*		3.5	1 90. drop							2.8	90. light	*						3.0	1
91. Religion	NC	91. Bible	*						2.1	1 91. belief							2.7	91. belief							2.9	
92. Scissors	N	92. cut							2.1	92. cut							2.8	92. cut							3.1	
93. Fine	N	93. coarse							1.9	93. find		*					3.5	1 93. coarse		*					3.3	1
94. Bath	N	94. soap							2.6	94. soap							2.2	94. soap							2.8	
95. Pull	N	95. push							1.7	95. lift							2.6	95. push							2.7	
96. Street	CAg	96. city							2.0	96. city							2.7	96. stores		*					3.8	1
97. King	N	97. queen							2.3	97. kingdom							3.3	97. queen							2.3	
98. Self-resp.	C	98. ideal					*		4.4	1 98. ideal							4.2	98. no good	*						2.6	1
99. Cheese	N	99. butter							2.6	99. butter							3.3	99. butter							2.8	
100. Grace	N	100. awkward							3.2	100. thank					*		4.1	1 100. thanks							3.4	
									Mean								Av.								Av.	
C (10 words)			0	0	0	8	1	5.2	5		0	0	0	1	1	2.0	2		0	0	0	0	0	0	2.8	0
CA1 (15 words)			0	1	0	0	0	2.5	1		0	1	0	0	0	2.5	1		12	3	0	1	2	2	3.2	16
CAg (6 words)			0	1	0	0	1	3.2	2		0	0	0	0	1	3.1	1		0	2	0	0	1	0	3.3	2
NC (7 words)			0	4	0	2	0	3.2	6		0	0	0	1	1	3.2	2		0	0	0	0	0	0	3.2	0
CA1+NC (8 words)			0	0	0	1	1	5.4	2		0	0	0	0	0	2.8	0		3	0	0	0	0	0	3.6	3
N (52 words)			0	2	0	5	2	3.4	7		0	4	0	2	2	3.6	8		0	5	0	0	1	1	2.9	6
Totals (100)			0	8	0	16	5	3.6	23		0	5	0	4	5	2.7	14		20	10	0	1	3	3	3.0	33
								SD								SD									SD
								1.1								.75									.64

TABLE 1

Session No. IV Waking Complex 8-9-32 4:50 P.M.	Disturbances: Verbal	Voluntary	Involuntary	Respiration	Delayed R.T.	Individual Reaction Times	Total
Response							
1. chair						1.7	
2. square						2.2	
3. radio						2.3	
4. bat						1.8	
5. woman						2.0	
6. peculiar						2.7	
7. fibre						4.7	
8. food						3.1	
9. light						3.9	
10. resort		*				5.5	1
11. white						1.4	
12. hard						3.2	
13. sleep						4.2	
14. foot		*				5.0	1
15. apple						2.0	
16. clothes		*				4.6	1
17. wings		*				5.2	1
18. rough		*				4.9	1
19. hug						5.0	
20. table						2.4	
21. sour						2.5	
22. pull						2.4	
23. girl	*	*				4.5	2
24. sleep				*		4.0	1
25. color						2.5	
26. hope						3.2	
27. deep						2.8	
28. silk	*	*				5.5	2
29. girl						2.8	
30. room						3.2	
31. light						3.2	
32. city						3.4	
33. hand						2.5	
34. vibration				*		4.3	1
35. thread						1.7	
36. long						2.5	
37. food						3.6	
38. blow						2.7	
39. swift						3.8	
40. package		*			*	6.1	2
41. low						2.6	
42. labor						3.3	
43. animal						2.3	
44. brown	*		*			3.4	2
45. smooth						2.9	
46. duty				*		4.4	1
47. head						2.5	
48. soft						1.8	
49. combustion		*			*	6.8	2
50. dream						2.4	
51. river						2.0	
52. boy	*	*		*		5.9	3
53. taste		*				4.1	1
54. courage						4.0	
55. intercourse						2.5	
56. bird						2.2	
57. girl						2.3	
58. no		*			*	7.3	2
59. sick						2.1	
60. push						2.1	
61. distasteful	*					5.0	1
62. water						2.5	
63. light			*			4.8	1
64. small			*			2.2	1
65. forget						2.5	
66. burn	*					2.5	1
67. food				*		5.0	1
68. me	*	*	*		*	6.4	4
69. blue						3.2	
70. electricity						2.8	
71. punishment						4.0	
72. short						1.7	
73. fast		*				2.9	1
74. girl	*					2.4	1
75. round		*		*		5.8	2
76. memory	*	*	*			3.9	3
77. burn	*					2.4	1
78. nail		*				1.9	1
79. street						2.4	
80. head		*		*	*	6.1	3
81. bread						2.8	
82. sky						1.5	
83. light						2.5	
84. table						2.4	
85. dishonorable	*			*		4.5	2
86. regret	*					3.3	1
87. sleep						2.1	
88. burn	*					3.8	1
89. cigarette		*	*	*		4.7	3
90. spark	*					2.5	1
91. belief						2.7	
92. cut			*			3.2	1
93. coarse		*				4.9	1
94. soap						2.9	
95. push						2.1	
96. display				*		2.0	1
97. kingdom						2.5	
98. dishonorable	*			*		2.8	2
99. butter	*					2.2	1
100. thanks						3.7	1
						Av.	
	5	5	2	0	3	3.45	15
	7	4	3	2	0	3.5	16
	0	2	1	2	2	5.0	7
	0	1	0	0	0	3.8	1
	2	2	1	0	1	4.3	6
	0	5	5	0	0	2.8	15
	14	20	12	4	6	3.3	60
						SD	
						1.3	

Session No. V Waking Control 3-10-32 3:50 P.M.	Disturbances: Verbal	Voluntary	Involuntary	Respiration	Delayed R.T.	Individual Reaction Times	Total
Response							
1. chair						1.6	
2. cube						2.1	
3. piano						2.0	
4. bat						1.5	
5. woman						1.5	
6. peculiar						1.6	
7. fine						2.0	
8. chewing						2.6	
9. light						1.5	
10. pertinent						2.1	
11. white						1.6	
12. hard						2.2	
13. sleep						2.3	
14. foot						1.5	
15. apple						1.9	
16. clothes						2.0	
17. insect						1.7	
18. rough						1.7	
19. hug				*	*	5.1	2
20. table						1.5	
21. sour						1.6	
22. pull						1.8	
23. man						1.6	
24. sleep						1.8	
25. color						2.3	
26. hope						2.0	
27. deep			*	*	*	2.7	3
28. white						1.7	
29. nature						2.5	1
30. light						1.9	
31. generator						2.1	
32. people						2.4	
33. hand						2.0	
34. chords						2.9	
35. thread						1.8	
36. long						1.6	
37. intestines				*		2.7	1
38. blow						1.7	
39. fast						1.4	
40. smoke						1.7	
41. low						1.4	
42. labor						2.0	
43. animal						1.5	
44. black						1.6	
45. smooth						1.3	
46. errand						2.0	
47. vegetable						2.2	
48. soft						1.6	
49. combustion						2.7	
50. dream						2.1	
51. river						1.9	
52. boy						2.4	
53. sweet						1.8	
54. brave						1.8	
55. home						2.0	
56. bird						1.7	
57. girl						1.6	
58. innocent						2.0	
59. sanitarium						2.2	
60. part					*	5.6	1
61. brain						2.7	
62. water						1.9	
63. book						2.1	
64. house				*		2.8	1
65. think						2.0	
66. burn				*		2.1	1
67. food						1.9	
68. accuse					*	4.9	1
69. sea						3.0	
70. lamp						3.1	
71. torture				*		7.1	1
72. short						2.2	
73. slow						1.3	
74. appeal						2.9	
75. round						1.5	
76. sweet						1.8	
77. ruin						1.8	
78. nail						1.5	
79. street						2.3	
80. head					*	5.4	1
81. milk						2.2	
82. sky						1.6	
83. light						1.7	
84. table						1.7	
85. worthless						2.8	
86. sorrow						2.2	
87. sleep						1.8	
88. olfactory						3.3	
89. cigarette						2.4	
90. hurt					*	5.1	2
91. belief						1.8	
92. cut						1.7	
93. coarse						2.4	
94. soap						1.8	
95. push						1.6	
96. city						1.9	
97. kingdom						1.4	
98. self-esteem						2.3	
99. butter						2.7	
100. thanks						2.0	
						Av.	
	0	0	0	0	2	2.8	2
	1	1	0	0	0	2.1	2
	1	0	0	0	1	2.8	3
	0	1	0	1	0	2.1	2
	1	0	0	0	0	2.1	1
	1	8	0	1	2	2.8	14
						SD	
						.88	

Session No. VI Hypnotic Control 3-10-32 4:10 P.M.	Disturbances: Verbal	Voluntary	Involuntary	Respiration	Delayed R.T.	Individual Reaction Times	Total
Response							
1. chair						1.5	
2. cube						1.8	
3. sheet						2.2	
4. bat						1.5	
5. woman						1.5	
6. peculiar						1.9	
7. fine						1.9	
8. chewing						2.0	
9. light	*					1.7	1
10. important	*				*	3.3	2
11. white						1.4	
12. hard						1.7	
13. dream	*		*			2.8	2
14. foot	*					2.3	1
15. apple						1.8	
16. clothes						1.9	
17. insect	*					2.1	1
18. rough						1.7	
19. hug						1.9	
20. table						1.9	
21. sour						1.8	
22. pull						1.7	
23. man						1.6	
24. sleep						2.0	
25. color						2.1	
26. hope						1.8	
27. deep						1.6	
28. white						2.4	
29. nature						2.3	
30. light						2.0	
31. generator						2.4	
32. people						1.8	
33. hand						1.7	
34. vibration						2.7	
35. thread						1.8	
36. long						1.6	
37. food						2.0	
38. blow						1.8	
39. swift						1.9	
40. smoke						1.9	
41. low						1.7	
42. labor						1.8	
43. animal						1.8	
44. black						2.2	
45. smooth						1.5	
46. errand						1.9	
47. head						1.8	
48. soft						1.6	
49. smoke						2.3	
50. dream						1.7	
51. river						1.6	
52. boy						1.3	
53. sweet						1.3	
54. fight						2.0	
55. home						1.7	
56. bird						1.6	
57. girl						1.3	
58. innocent						1.6	
59. poor						2.0	
60. deep						2.1	
61. thought	*					2.5	1
62. water						1.7	
63. book						1.5	
64. house						1.5	
65. think						1.7	
66. light						1.8	
67. stomach						2.0	
68. accuse					*	3.3	1
69. sea						1.8	
70. window						2.3	
71. punish						2.6	
72. short						1.4	
73. slow						2.0	
74. appeal						2.4	
75. round						1.5	
76. sweet						1.5	
77. ruin						2.0	
78. nail						1.8	
79. street						1.6	
80. head						2.2	
81. milk						1.8	
82. sky						1.5	
83. light						1.5	
84. table						1.7	
85. useless						2.4	
86. sorrow						1.5	
87. sleep						1.7	
88. taste						2.4	
89. smoke						1.9	
90. drop						1.8	1
91. belief						1.7	
92. cut						2.2	
93. coarse						1.7	
94. tub						2.3	
95. push						1.8	
96. city						2.0	
97. kingdom						2.2	
98. self-esteem						2.8	
99. butter						1.8	
100. thanks						2.4	
						Av.	
	0	0	0	0	1	2.3	1
	0	0	0	0	0	2.0	0
	0	1	1	0	0	1.9	1
	0	2	0	1	0	1.9	3
	0	1	0	0	0	1.5	1
	0	1	0	0	1	1.8	2
	0	7	0	1	2	1.9	10
						SD	
						.36	

Session No. VII Hypnotic Control 3-11-32 4:45 P.M.	Disturbances: Verbal	Voluntary	Involuntary	Respiration	Individual Reaction Times	Total
Response						
1. chair					2.1	
2. cube					1.7	
3. sheet					1.9	
4. bat					1.5	
5. woman					1.5	
6. peculiar					1.6	
7. fine					2.0	
8. chewing					2.1	
9. light					1.4	
10. sure					2.6	
11. white					1.6	
12. hard					1.9	
13. sleep					2.2	
14. foot					1.7	
15. apple					1.6	
16. clothes					1.8	
17. insect					1.7	
18. rough					2.0	
19. hug					1.8	
20. table					1.5	
21. sour					1.6	
22. pull					1.6	
23. man					1.9	
24. ease					1.5	
25. color					1.9	
26. hope					2.0	
27. deep					1.7	
28. white					1.4	
29. pretty	*	*			2.2	2
30. light					2.3	
31. generator					2.1	
32. people					2.3	
33. hand					2.0	
34. noise					2.6	
35. thread					2.2	
36. long					1.5	
37. food					2.8	
38. blow	*				1.9	1
39. swift					2.0	
40. smoke					2.1	
41. low					1.5	
42. labor					1.9	
43. animal					1.4	
44. black					2.2	
45. smooth					1.5	
46. errand					2.0	
47. head					2.8	
48. soft					1.9	
49. smoke					2.8	
50. dream					3.1	
51. river					1.7	
52. boy					1.4	
53. sweet					1.4	
54. fight					2.0	
55. home					1.8	
56. bird					1.8	
57. girl					1.4	
58. innocent					2.2	
59. poor					2.3	
60. deep					3.0	1
61. thought		*			2.7	1
62. water					1.9	
63. book					1.9	
64. house					2.1	
65. think					1.6	
66. light					2.4	
67. food					2.3	
68. accuse					3.1	
69. sea					1.9	
70. window					2.8	
71. punish					2.6	
72. short					1.7	
73. slow					2.2	
74. male & fem.					3.0	1
75. round					1.7	
76. sweet					1.9	
77. ruin					2.0	
78. nail					1.7	
79. street					1.8	
80. head					2.0	
81. milk					2.2	
82. sky					1.6	
83. light					1.7	
84. table					2.0	
85. no good					3.3	1
86. sorrow					1.7	
87. sleep					2.0	
88. burn	*				2.0	1
89. smoke					2.7	
90. drop					2.5	
91. belief					2.0	
92. cut					1.6	
93. coarse					2.6	
94. tub					2.1	
95. push					1.5	
96. city		*			2.5	1
97. kingdom					2.1	
98. self-esteem					3.2	
99. butter					2.1	
100. thanks					2.7	
					Av.	
	1	0	0	1	2.3	2
	1	0	0	0	2.2	2
	1	1	0	0	2.1	2
	0	1	0	0	1.3	0
	0	0	0	0	1.8	0
	0	1	0	0	2.0	1
	2	4	0	2	2.0	9
					SD	
					.46	

TABLE 1 (*continued*)

The verbal responses in all sessions and the disturbances which were associated with them are presented in Table 1. A careful perusal of this table will show the marked change which the verbal responses underwent in Sessions III and IV, especially those classed as C and CA_1. No additional reference will be made to these responses except in special instances. Each session is summarized at the bottom of the table for types of disturbance appearing on the word classes.[14] It is at once apparent that the totals for the complex sessions show a greater number of disturbances than the totals of the control sessions, the hypnotic complex having 38, and the waking complex 60, as opposed to 14 and 21 of the respective control sessions. This increase is largely due to those word classes designated as C and CA_1. In Session III—the hypnotic complex session—27 of the 38 disturbances are accounted for by these groups, and in Session IV—the waking complex session—31 of the 60 fall in the C and CA_1 classes. The induction of the complex has apparently had two effects: to "set" the subject so that he gave verbal replies which were related to the complex and to disorganize his voluntary, involuntary, and respiratory behavior. In addition, these effects manifest themselves differently in the hypnotic and waking states. Of the 27 disturbances accounted for by C and CA_1 words in Session III 21 are verbal, while in Session IV but 12 of the 31 disturbances are verbal. Reference to Table 2 shows this clearly. In hypnotic Session III, 84 per cent of the C and CA_1 words were disturbed verbally and their non-verbal disturbance was 5.3 per cent.[15] In waking complex Session IV, the verbal disturbance has

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1.5 times the S.D., which we set as our criterion, fell outside the limits of the distribution—this attempt was given up. We finally took those reaction-times which seemed to stand apart from the rest of the distribution. Usually this gave four or five disturbed reaction-times in a session.

[13]We recognize that in using the word "disturbance" for both verbal and non-verbal effects we are not being consistent. In the former case we are using the word in the sense of a *change* in response in the direction of the complex, in the latter in the sense of a *disorganized* response. However, since the term is convenient we are continuing its use in both senses.

[14]A refined statistical treatment of the data was attempted but we came to the conclusion that this was premature because of the qualitative stage of the technique and the exploratory nature of the experiment.

[15]These percentage figures are arrived at by dividing the actual number of disturbances by the possible number of disturbances. Since there are 25 C and CA_1 words, the possible verbal disturbance is 25. However, since each word may be disturbed non-verbally in the voluntary, involuntary, and respiratory aspects, there are 75 possible non-verbal disturbances. The

TABLE 2

PERCENTAGE VERBAL AND NON-VERBAL DISTURBANCES FOR WORD CLASSES (*C* & *CA*$_1$ COMBINED) AND FOR ALL OTHERS (*CA*$_1$+*NC*, *NC*, *CA*$_2$, *N*)

	Session I Waking control				Session II Hypnotic control				Session III Hypnotic complex			
	Verbal		Non-verbal		Verbal		Non-verbal		Verbal		Non-verbal	
	D	%	D	%	D	%	D	%	D	%	D	%
C & *CA*$_1$	0	0.0	3	4.0	0	0.0	2	2.7	21	84.0	4	5.3
All others	0	0.0	13	5.8	0	0.0	7	3.2	3	4.0	7	3.1

	Session IV Waking complex				Session V Waking control				Session VI Hypnotic control			
	Verbal		Non-verbal		Verbal		Non-verbal		Verbal		Non-verbal	
	D	%	D	%	D	%	D	%	D	%	D	%
C & *CA*$_1$	2	48.0	16	21.4	1	4.0	1	1.3	0	0.0	2	2.7
All others	2	2.7	24	10.7	0	0.0	9	4.0	0	0.0	6	2.7

	Session VII Hypnotic control			
	Verbal		Non-verbal	
	D	%	D	%
C & *CA*$_1$	2	8.0	2	2.7
All others	0	0.0	2	0.9

D=Number disturbed

Possible total verbal disturbance	*C* & *CA*$_1$	25
Possible total verbal disturbance	All others	75
Possible total non-verbal disturbance	*C* & *CA*$_1$	75
Possible total non-verbal disturbance (Reaction-time excluded)	All others	225

fallen to 48 per cent on the class C and CA_1 taken together, while the non-verbal has risen to 21.4 per cent.

The following explanation of this result occurs to us. Since the complex was given under hypnosis, it is reasonable to assume that the resulting mental set would cause the subject to reply to the C words with a response which is related to the complex story. That this set was of considerable importance is seen by the other words (CA_1) which were drawn into the complex situation, there being 15 such.[16] In the waking state, however, the subject, as nearly as we could determine, had an amnesia for the complex and was therefore probably in lesser contact with it. At the same time he was in greater rapport with the experimental environment, hence the verbal responses related to the complex were not elicited as easily. To explain the small number of non-verbal responses in the hypnotic complex session, there being 4 on C and CA_1 words (excluding reaction-time), we might assume that whatever excitation the conflict created was discharged verbally in giving the related response. In the following waking complex Session IV, however, the verbal responses related to the complex were not elicited as readily and the excitation created by the conflict spread into the motor response, giving rise to greater disturbances. Both C and CA_1 words illustrate this point. In the hypnotic complex Session III there were 9 responses on the C words related to the complex and none on the non-verbal. But in the waking complex Session IV there were but 2 which were disturbed verbally alone, 3 both verbally and non-verbally, and 3 non-verbally. On CA_1 words in the hypnotic complex Session III the results are: verbal alone 9, verbal and non-verbal 3, non-verbal alone 0; in the waking complex Session IV: verbal alone 4, verbal and non-verbal 3, and non-verbal 3. The further implication of these results is that, while the combined association-motor method of Luria does seem to reveal conflicts induced by means of hypnosis, a conflict need not necessarily result in disorganization at the non-verbal level. It would be difficult otherwise

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total possible non-verbal disturbance on all other words together is 225. Reaction-time is excluded since the standard we used was relative to the whole distribution of reaction-times, so that only a few long times for each session could be marked as disturbed.

[16]The actual number probably depends upon many factors such as the strength of the complex, the nature of the complex itself, its relation to the stimulus words, and personality traits.

to explain the paucity of non-verbal disturbances in the hypnotic complex session. It seems that some such assumption as we have indicated concerning the possibility of a complete discharge of excitation verbally is necessary.[17]

But how shall we account for the other disturbances which have appeared in these two sessions? Indeed, what is the meaning of the disturbances which occur in the control sessions? In Session III there were 3 verbal disturbances on the word Class CA_1+NC which have not been discussed thus far. It is clear that the verbal disturbance is due to the CA_1 and not to the NC since our criterion of classifying them CA_1 was based on relationship to the complex story. No non-verbal disturbances appear in this class.[18] Two voluntary disturbances appear on the class designated as CA_2 on Nos. 9 and 96. These are probably related to the complex as already indicated by the way in which they were selected. The remaining

[17]It is possible to urge that the subject had little or no conflict in the hypnotic state because the complex was given to him in this condition. Thus, by virtue of having heard the story, without assuming personal responsibility for the act, he was set to give verbal responses related to the complex with no affect necessarily associated with them. It should be noted, however, that an attempt was made, in the hypnotic instructions which were used when the complex story was told to the subject, to associate affect with the story. It was repeatedly emphasized to the subject that he had committed an act which made him feel miserable. The subjects in general were restless after the induction of the complex and gave one the impression that something distressed them.

It is interesting also to examine the verbal reaction-times of the C and CA_1 words in Session III. The mean of the C words is 2.6 seconds, being shorter than the mean of the 100 words, which was 3.0 seconds. None of these words carried any non-verbal disturbances. A reaction-time mean on the C words which is close to the total mean of the whole stimulus word-list is consistent with both the levels-of-discharge hypothesis and the "non-affective set" view. That is, there is no reason in either case why any delay should take place. The reaction process simply runs its normal course, discharging more affective excitation in the former case than in the latter. The CA_1 words have a mean of 3.2 seconds. Of the 15 words in this class, 12 have verbal disturbances. It is interesting to note that 3 of these 12 have non-verbal disturbances, words No. 45, 48, and 85, and each one has a long reaction-time, the values being 4.4, 5.1, and 5.0 seconds respectively. This result would seem to imply that there was inhibition of the verbal response which was reflected in the non-verbal aspects as disturbances. The motor responses give some evidence of conflict in the hypnotic state. Unfortunately there are only a few instances in this subject so that the question can not be answered decisively. In some other cases the issue is clearer, however, and we shall return to this topic later.

[18]We have kept the class separate, however, since there are some non-verbal disturbances in other sessions which may be due to NC.

6 disturbances are on 5 N words.[19] These are numbers 36, 37, 59, 65, and 93. No. 37 ("Stomach—food") with a voluntary dis-

[19]There are numerous disturbances which are not accounted for on the basis of a relationship to the induced complex or to natural complexes. One might attempt to explain these disturbances by the following methods. Often it is impossible to make a satisfactory decision. Some of these methods apply to all sessions and some only to particular sessions. Those which apply to *all* sessions are the following: (1) Perseveration effects (disturbances appearing on words following affective stimulus words) which may be both verbal and non-verbal or either one separately. If the perseveration is verbal and the subject gives a response which is related to the complex then it would be classed CA_1, but if it is unrelated we would be unable to tell whether or not it is a case of perseveration. There is the possibility, however, that perseveration may make the verbal response unusual. The clearest example of perseveration in a non-verbal disturbance would be that which follows a verbal or non-verbal disturbance and which appears on an *N* word. (2) Cases of unusual stimulus words to which the subject has difficulty in giving a response, or cases where the subject inhibits the first response which occurs to him but which is not connected with a conflict. Here one would expect longer reaction-times. (3) Transitory "natural-complexes"—those which have arisen just prior to a particular experimental session and which were not present at the next session. (4) Chance disturbances due to movements of the subject because of physical discomfort of some kind. One may raise the question here of the relationship of such movements to affective stimulus words. Our data on this point are not sufficient since a semi-opaque curtain separated the subject and the experimenter through which only gross movements of the former could be detected. The subject was unable to see the experimenter because the latter's corner of the room was darkened. (5) Natural complex situations unknown to us.

The explanations which apply to *particular* sessions are the following: (1) Responses which are related to the complex, but which we did not detect. These could be *C*, CA_1, or CA_2; *C* and CA_1, in those cases in which the response was the same as that of the first control sessions but in which the subject ascribed new meaning to the stimulus word, relating it to the complex; CA_2, in which we were unable to establish the associative links. This should apply especially to complex sessions and to some extent to control sessions after the removal of the complex (see 5 below). (2) "Natural Complex" situations, the thresholds of which have been lowered by the artificial complex so that words not marked *NC* become disturbed non-verbally. This should apply primarily to complex sessions. (3) Disturbances which are reactions to hypnosis itself and not the complex as such. These might be due to two different factors. The words which were used by the hypnotist in putting the subject into the trance, e.g., "Sleep," might become disturbed if the subject had developed any antagonism toward the hypnotist because the unpleasant complexes were suggested in hypnosis. The other factor is the possibility of ambivalence toward hypnotism. One would expect such disturbances to appear largely in the waking complex sessions and perhaps in later waking control sessions, since in the waking state rapport is probably not as good as in the hypnotic state. (4) Disturbances due to "shock"—those arising from the subject's apprehension in a new situation in which he does not know entirely what is expected of him. This is mainly relevant for the first session. One specu-

turbance, and No. 59 ("Health—sanitarium") also with a voluntary disturbance may both be considered as indicative of some natural complex material since the subject reported he was taking a tonic for stomach trouble at the time of the experiment. This rereceives some confirmation from No. 67 ("Hungry—sick"). On the other hand, it is possible that this group of words implies that the subject felt miserable—a "sick to the stomach" feeling—because of his behavior in the complex story. Such an interpretation finds support in the somewhat greater unpleasant affective tone of the session as compared with the first two control sessions.[20] Nos. 37 and 59 may also involve an element of perseveration from 36 and 58 respectively. The disturbances on No. 65 ("Concentrate—meditate") —voluntary and reaction-time—may have arisen because of the subject's preoccupation with the complex story. On No. 93 ("Fine—coarse"), disturbed in the voluntary aspect, the response which appeared in a previous control session may have taken on a figurative meaning for the subject, referring to his behavior in the complex situation as "coarse." We are unable to account for the voluntary disturbance on No. 36 ("Short—long") satisfactorily.

We have attempted to account for all the disturbances in the other sessions according to the principles set forth above. However, since we do not feel these explanations are pertinent to our present purpose, we shall give but a few additional examples.

The waking complex Session IV has 15 disturbances on 14 *N* words: Nos. 10, 24, 34, 46, 53, 64, 67, 73, 75, 92, 93, 99, and 100. No. 53 ("Sour—taste")—this is the only time in which "taste" appears as a response to this stimulus word. This fact in combination with the appearance of an involuntary disturbance suggests that the subject was referring to the unpleasantness of the complex in a figurative sense. There may also be an element of perseveration from No. 52 involved which also showed considerable

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lates in this connection whether or not apprehensiveness in a new situation sensitizes the "natural complexes" of the individual. (5) In the control sessions after the complex removal one has the possibility that there are residuals of the complex. Also, if there has been an enhancement of the subject's natural conflicts, disturbances on *NC* words might be found here.

In attempting to account for disturbances falling on *N* words we shall point out which of the above explanations seem the most likely to us in so far as we have any basis for our conclusions.

[20]An analysis of the verbal responses of the complex sessions indicates that there has been such a shift toward unpleasantness from the first control session, cf., e.g., in Session III, Nos. 38, 48, 61, and 98.

disturbance. No. 73 ("Swift—fast")—the response "fast" which is a different type of response from that of the control sessions a synonym as opposed to an antonym, appears with a voluntary disturbance. We may interpret this as probably referring to the necessity of speed in notifying the girl of the truth, a point touched on in the complex story.[21]

Having considered the complex sessions we shall take up the control sessions, beginning with Session I. In this first waking control session there are 21 disturbances on seventeen words. It is especially interesting to note that there are six disturbed *NC* words out of a possible seven—a fact which would seem to indicate that *NC* material may be turned up, at least the first time the test is given. These *NC* disturbances are on Nos. 19, 32, 52, 55, 90, and 91.

The possible explanation that may be given to the other disturbances is that they are the results of natural complexes, permanent or transitory, of which we were not aware (enhanced or unenhanced by a "shock" effect), chance disturbances due to "shock," somewhat unusual stimulus words for the subject, or perseveration. No. 68 ("Blame—accuse") with a respiratory disturbance, and No. 71 ("Persecute—electric chair") with voluntary and reaction-time disturbances are probably disturbed because the stimulus words are somewhat unusual and often carry an unpleasant tone.

In Session II, hypnotic control, we should expect the "shock" effect of Session I to be reduced, but since hypnosis may reach a deeper personality level, the possibility of more natural complex material being aroused should be considered. The total disturbance in Session II has fallen to 14 and appears on words 2, 10, 27, 41, 49, 54, 58, 67, 86, 93, and 100. Word No. 41, with disturbances of respiration and reaction-time, is *NC* and No. 67 ("Hungry—stomach") with a voluntary disturbance may also have been connected with a natural complex as has been already indicated. As for No. 49 ("Burned—chemistry")—respiration and reaction-time disturbed—we have a post-experimental report of severe acid burns received by the subject at one time which may account for the present disturbance. No. 54 ("Soldier—fight"), having a voluntary disturbance, may be connected with the natural complex in-

[21]Another interesting point with regard to Session IV is that the subject showed one of the general behavioral characteristics which we were observing. On No. 89 ("Tobacco—cigarette"), a CA_1 word, the subject sighed deeply.

volving "Fall" and "High" since the subject's aeroplane accident, already mentioned, took place while he was in the Army Air Service.

In Session V, the first waking control after the complex session, we find 14 disturbances which fall on Nos. 19, 27, 29, 37, 60, 64, 66, 68, 71, 80, and 90. Any analysis must now consider the possibility of verbal and non-verbal affective residuals, and non-affective verbal residuals from the complex sessions. No. 19 ("Kiss—hug"), disturbed in the voluntary and respiratory aspects, is *NC,* and it is probable that the *NC* part of No. 90 is responsible for the voluntary disturbance on it since the verbal response is "hurt." The verbal disturbance on No. 66 ("Lamp—burn") is probably due to a verbal affective or non-affective residual.

The disturbances fall to 10 in the hypnotic control Session VI—on Nos. 9, 10, 13, 14, 17, 61, 68, and 90. Nos. 13, 17, and 90 are *NC* words.

Session VII shows 9 disturbances on Nos. 29, 38, 60, 61, 74, 85, 88, and 96. The non-verbal disturbance of a voluntary nature combined with the verbal responses on No. 29 ("Beautiful—pretty") suggests an affective residual. The verbal disturbance on No. 88 ("Smell—burn") suggests either an affective or non-affective residual.

The attempt to account for disturbances not related to the induced or natural complexes has, in the very nature of the case, to be quite speculative. We included examples of these interpretations because it seemed to us to point out the problem which future experimentation must face in order to make the technique a really satisfactory one.

Reference to Table 1 shows the mean reaction-times and S.D.'s for all sessions, and the means for the word classes. Examination of the means and S.D.'s of all the sessions, despite questions which might be raised about the legitimacy of this measure of variation in this case, shows several interesting points. While the means of the first two controls are practically identical, the S.D. of Session I is somewhat larger than that of Session II, which may be an expression of what we have referred to as the "shock" effect. The word classes which contribute largely to this larger S.D. in Session I are CA_1+NC, *NC,* CA_2, and *C,* in order of their departure from the total mean from great to small. Since in the first control sessions the CA_1+NC and *NC* groups are really all *NC,* it seems clear that there is a tendency for *NC* words in this session to be delayed. The

longer than mean time on the CA_2 class may be caused by some natural complex material. Word No. 71 ("Persecute—electric chair") and its response suggests this inference. Since the *C* words, in general, were a bit unusual, this might lengthen their time. The class means have come together considerably in Session II, the *NC,* however, still being the longest. The mean of Session III is slightly greater, but the S.D. has continued to fall, the most surprising fact being the short reaction-time of the *C* words, 9 of which were disturbed verbally. This seems to support the verbal discharge hypothesis presented earlier, and is in line with Luria's "law of the catalytic action of the stimulus." In the waking complex Session IV the mean has continued to rise, being greatest of any session, and the scatter has increased. The increases are on CA_2, *C,* and CA_1 words, which is to be expected if there has been any tendency to inhibit verbal responses. After the removal of the complex the means and S.D.'s decline to new low levels, probably due to a practice effect (7). In none of the sessions may the differences between means be regarded as significant statistically. Rather it is their general agreement with what might be expected which is striking.

Another point which might be mentioned is that there was a tendency for responses which were more highly specific to the complex to appear on the ten "Complex" words and for those which were less specific to appear on "Complex-Associated" words. This tendency was not paralleled in the magnitude of the voluntary disturbances.

Summary of Results on All Subjects

Twelve subjects went through the same general procedure. There is evidence that nine accepted the story told to them as an account of something they had done. We base this on their general behavior during and between experiments, an example of which we have given in discussing the sample case.

Table 3 summarizes the results on all cases. Since there is a considerable amount of overlap, the plus (+) signs should be regarded only as indicative of predominant trends. In the waking state non-verbal behavior was disturbed in Subjects 1, 3, 4, 5, and 6*c;* in the hypnotic state in Subjects 2, 3, 4, 5, 6*a,* and 6*b*. The results on Subjects 7, 8, and 9 are largely negative so far as dis-

TABLE 3
RESULTS ON ALL SUBJECTS

Subject		Complex accepted	How revealed as disturbances					
			Verbal only		Both verbal and non-verbal		Non-verbal only	
No.	Sex		H	W	H	W	H	W
1	M	Yes	+			+		
2	F	Yes			+			
3	F	Yes			+			+
4	F	Yes			+	+		
5	F	Yes			+	+		
6a	F	Yes			+			
6b		Yes			+			
6c		Yes			+	+		
7	M	Yes	+					
8	F	Yes	+	+				
9	F	Yes	+	+				
10	M	No						
11	M	No						
12	F	No						

H=Hypnotic Complex Session
W=Waking Complex Session

organization of the non-verbal aspects are concerned. Of the nine subjects who accepted the complex (Table 3), six gave definite evidence that some aspect of the Luria technique revealed the presence of a conflict. One of this group (No. 2) presented disturbances which were almost all of a voluntary character in hypnosis, with almost none in the waking complex sessions, so that it would have been difficult on the basis of the latter alone to know whether the subject had a conflict. However, after reenforcement of the suggestion, a few verbal and non-verbal disturbances appeared in the next waking complex session. This implies that this type of conflict may be entirely at the hypnotic level but if sufficiently strong may

result in disturbances in the succeeding waking session. Two subjects (Nos. 3 and 4) showed voluntary disturbances in both states. One subject (No. 5) had greater involuntary responses than voluntary in hypnosis and waking, and the subject (No. 1) whom we discussed in detail showed his greatest non-verbal disturbance as voluntary in the waking state. Another subject went through the experiment three times: in the first two (Nos. 6*a* and 6*b*) the disturbances appeared in hypnosis as voluntary, the third time (No. 6*c*) voluntary disturbances were found in both states.[22] It is difficult to generalize about these six cases, because such marked individual differences are found among them. However, one of the consistent findings is that the preponderance of verbal disturbances occurs in hypnosis and the relative importance of the non-verbal increases in the waking state. Another is that the *C* words, as would be expected, tend to evoke verbal responses which are more specifically related to the complex than any others.

Of the other three subjects who accepted the complex, in two the evidence that there was any connection between the complex and

[22]In this subject the instructions were altered the third time the experiment was performed. Instead of permitting her to have a post-hypnotic amnesia for the complex story, which was the usual technique, we told her while hypnotized that she would be aware of the complex post-hypnotically but would not dare to think about it although it would cause her to worry greatly. The increase in voluntary disturbances would seem to imply, if the instructions were followed, that the subject must be aware of the complex to manifest such disturbances. Luria has also commented on this point (5, pp. 149-161), maintaining that if the complex is removed from consciousness it is also insulated from the motor area since the two have been combined functionally by the technique. On the other hand, in the other five successful cases post-hypnotic amnesias were present and all of them (one after re-enforcement) showed a rise of non-verbal disturbances in the waking complex sessions. When these sessions were completed the subjects often manifested surprise about some of their verbal responses which related to the complex. Luria has also noted this (5, p. 136). Whether the surprise of the subjects was genuine or was in the nature of rationalizations based upon some knowledge of the complex we are unable to state definitely. Our impression is that the surprise was real. Luria, however, argues that each critical stimulus word makes the complex conscious, at least partially, and hence disrupts the motor responses. His evidence in this connection is based on continuous free association experiments in which the subject eventually got around to giving responses related to the complex. These were accompanied by motor disturbances. Our material indicates that both conscious and unconscious conflicts may be revealed by the technique. This question is probably connected with the strength of the conflict and the intimacy of the relation between it and the motor response.

the non-verbal disturbances was of a dubious character. Each case presented some peculiarities of its own which are worth mentioning. One subject (No. 7) who had eight sessions—two complex sessions, one hypnotic and one waking being added—had the largest group of words associated with the complex of any of our subjects. In the first hypnotic complex session there were 24 CA_1 words and 16 CA_1+NC words. Yet only three CA_1 and two CA_1+NC words were disturbed in the voluntary responses. One of each class had a voluntary disturbance without a verbal disturbance. Likewise there were 8 verbally disturbed C words, but only one had a voluntary disturbance. In the succeeding waking complex session there was one verbal disturbance on Class C and one on CA_1+NC. The C class had two voluntary disturbances and the CA_1 class had 5. In neither session was there an increase of non-verbal disturbance on the other word classes. In such a case it is important to examine the disturbances in the other classes and attempt to discover if these can be related to the complex, paying especial attention to those which were undisturbed in the control sessions. It was found that a few of these could be connected but the result was not very convincing. The results on the other two complex sessions were essentially the same except that the total disturbance declined somewhat. Another fact about this case was the large increase in non-verbal disturbances in the first hypnotic control session, as against the waking control, the numbers being 18 and 5, respectively. This suggests that there may be a "shock" effect which appears in hypnosis in some cases. This interpretation is supported by the fact that there were but two disturbances in the last hypnotic control and an increase in unusual responses in the first hypnotic control session over the previous waking control—the values being 10 and 2.

Another subject (No. 8) gave somewhat different results. Although there was a considerable amount of non-verbal disturbance in each session, there was little apparent relation to the complex. Verbal disturbances were few. In the hypnotic complex session there were found but 5—2 on C and 3 on CA_1 words. This was true, likewise, of the post-hypnotic complex session. The verbal responses were, however, sufficiently specific to the complex story to show its effect. The interesting point about this case is that from an examination of the total disturbances for each session there appears to be a "shock" effect in both the waking and hypnotic

states, greater in the former, and which has a differential rate of decline upon repetition, being faster in hypnosis than in the waking condition.

In another subject (No. 9) there were a considerable number of verbal disturbances in both the hypnotic and waking complex sessions but with few non-verbal disturbances which could be related to the complex situation in either the hypnotic or waking states. This individual had 12 sessions, 6 complex and 6 non-complex, divided equally between the hypnotic and waking states. Four control sessions preceded the induction of the complex and two were conducted after its removal. The control tests showed an interesting phenomenon—with repetition of the experiment there was a decline of disturbances. This was true of both the waking and hypnotic conditions and is better evidence for the existence of a "shock" effect than the decline in the case of those subjects in which but two controls were performed before the complex was suggested. There is also the possibility that this decline in disturbance is evidence of an "abreactive" effect. Failure to take this factor into consideration, whatever its cause, may lead to questionable conclusions. Luria, we believe, has not taken sufficient account of it in his studies of students, before and after school examinations, criminals, before and after confession of a crime, etc. (4, esp., pp. 43-128), and for that reason the differences may not be as marked as he indicates. Along with repetition went a slight reduction in reaction-time means and a shrinkage of scatter about these means. (This same general effect was noted in three other cases in which control repetitions were obtained.) Because of our failure to obtain many non-verbal disturbances we allowed this subject to keep the complex over night and repeated the hypnotic and waking sessions the following day. The result was a slight decline in total non-verbal and verbal disturbances. The next day we tried to re-enforce the complex by repeating the suggestion to the subject. This resulted in a slight increase in both types of disturbances, bringing them back to about their original level. (In another subject re-enforcement was tried with much more positive results, i.e., a great increase in disturbances, the effect being especially marked in hypnosis on non-verbal disturbances.)

It seems certain, then, that with repetition one has a reduction in disturbances during the complex sessions. An abreactive factor,

a forgetting factor, or a decrease in "shock" effect, must all be considered as possible causes for this decline.

The levels-of-discharge hypothesis which we have suggested in this paper is consistent with results obtained on all the subjects who accepted the complex. Stated broadly, the hypothesis implies that if affective excitation created by a conflict is not discharged completely at one level, it ought to appear at another. In terms of the present experiment if the affect was not totally released in giving a verbal response related to the complex there ought to be a non-verbal (voluntary, involuntary, or respiratory) disturbance. This may occur with or without a lengthened reaction-time. The reaction-time would be increased on those items in which there was some inhibition (conscious or unconscious) of the verbal response. The time would not be lengthened if a response related in some way to the conflict was either adequate or inadequate to discharge the affective excitation.

It should be noted that Subjects No. 2, 3, 4, 5, 6*a*, and 6*b* give motor evidences of a conflict in hypnotic states, a point which we discussed in connection with Subject No. 1 when we analyzed in detail the results obtained from him. The motor disturbances were much more marked in these cases than in Subject No. 1. Subjects No. 1, 3, 4, 5, and 6*c* gave similar evidence in the waking states. In the hypnotic states Subjects No. 1, 7, 8, and 9 did not give motor disturbances, and in the waking condition Subjects No. 2, 6*a*, 6*b*, and 7 did not show many non-verbal disturbances. It may be argued, as indicated previously, that the failure to give motor disturbances in the hypnotic or waking states in these subjects shows the absence of conflict.

The levels-of-discharge hypothesis we have advanced, on the basis of our experiments, is an extension of the Luria theory as we have stated it. Motor disturbances occur, according to Luria, when there is an inhibition of the verbal response, or when there are large amounts of conflict-excitation present. Our hypothesis assumes that motor disturbances will appear when there is inhibition of the verbal responses. However, motor disturbances may or may not be found when the conflict arouses large amounts of excitation. If the verbal response is adequate to discharge this excitation there will be no motor disturbances, but if the verbal response is inadequate then motor disturbances will appear. We have attempted to account for

the failure to obtain motor effects when the subject had a conflict in this way.

Of the remaining three subjects, two definitely refused to accept the complex (Nos. 10 and 11). Both were given the same complex, which was of a serious nature. As medical internes they were supposed (in the story suggested to them) to have been anxious to acquire proficiency in the technique of the cisterna puncture, practicing on patients who had just died. Through a mistake in the location of the bed in a poorly-illuminated ward, they performed the puncture on a comatose patient instead of a dead one, and due to faulty technique the vertebral artery was pierced by the needle. An internal hemorrhage resulted and death ensued. Each had gone from the ward without making a report of the accident. After we had finished the experiments we questioned the subjects and discovered that they had not believed that they were involved in the complex situation. One said that the behavior of the person in the story was so different from his own that he could not imagine himself as committing the action, although he tried hard to believe it. The other subject reported that after the suggestion of the story he had had a mental picture of himself performing a cisterna puncture but that this was entirely dissociated from the complex. The whole situation had seemed very real to him but he had known that he had not gone on the ward in the evening to perform the operation. Both of these cases, however, showed an increase in disturbances in the complex sessions. These disturbances fell almost entirely on words classed as *N* and in one case to a slight extent on *NC*. In one case the increases appeared largely as respiratory disturbances; in the other they were scattered among all the non-verbal aspects. Luria has spoken of an increase in respiratory disturbance as indicative of "trauma," by which he means those cases in which the person has reacted to a "shocking" experience. He contrasts these with those in which the individual has taken a part in some act which, if contrary to the personality trends of the person, leads to a conflict. The implication of the results in these two subjects is that to produce a conflict the complex act must be one possible for the subject to imagine his doing.

The third case (No. 12) in which negative results were obtained, from the standpoint of accepting the complex, involved the failure to mail a roommate's letter of application for a graduate school

fellowship until after the final date for application had passed, while the subject mailed one of her own, and obtained the same fellowship. This subject explained after the experiment that the account as stated had not been entirely logical and that she felt it was artificial. She had elaborated it with numerous details to make it more credible. In the post-hypnotic complex state she was aware of a desire to let her mind dwell upon events that might have happened in the trance state and that by so doing she thought she might rid herself of something unpleasant. Yet she felt vaguely that she ought not do this since it might disturb the hypnotic situation. Complex removal was accomplished in her in the waking state by suggesting that she recall the story, and this she was able to do. It is our impression in this case that the acceptance of the story was more intellectual than emotional. There was a slight increase in disturbances in complex sessions, appearing in the hypnotic state on *N* and *NC* words almost entirely. In the waking complex session there was a small increase over the hypnotic complex session caused by disturbances appearing on *C* and CA_1 words. This makes it appear that some conflict has been set up. The most striking thing about this subject, however, was the large number of unusual verbal responses. By unusual responses we mean those which are individual to the subject as compared to those expected from a group of persons living in a similar environment. In the case of the words which we took from the Kent-Rosanoff list we designated those responses as unusual which had a very low or zero frequency, unless the subject's occupation and habitat made the response seem appropriate. (In the subject whose case we presented in detail [Table 1] the response "sigh" to the stimulus "Whistle" No. 38 was considered as unusual. This was true also of the response "me" to the stimulus "Persecute" No. 71). The values ran as follows: Session I, waking control—7; Session II, hypnotic control—11; Session III, hypnotic complex—18; Session IV, hypnotic control—9. The majority of these fell on the word classes *NC* and *N*. An examination of the individual responses which appeared in Sessions III and IV which had not occurred in Sessions I and II showed that some could be accounted for as *NC,* some as related to the complex, and some as influenced by both. This analysis was made in the same way as in the attempt to account for all the disturbances in the sample case. Although the numbers dealt with are small, they suggest that the increase in

unusual responses may be augmented directly by the complex, or indirectly through the sensitization of *NC*.[23] [24]

Conclusions

The outstanding difficulty of the technique is the more or less speculative method which must be resorted to for the explanation of non-verbal disturbances not clearly related to the complex. For more exact experimental investigations one certainly should know more about the personalities of the subjects than we did. It is likely also that the list of neutral words would vary from subject to subject. An attempt should be made to secure subject-constancy rather than constant conditions for all subjects. This would make it possible to attack a number of problems on a more objective basis, some of which are here presented.

1. Must a subject be aware of a conflict to obtain voluntary disturbances? Our data offer arguments for but mainly against this possibility. We believe that Luria's point both ways also. However, he seems to hold for the necessity of awareness to obtain voluntary disturbances.[25] The problems dealing with repression and suppression of conflicts and Luria's concept of "functional barrier" are involved here.

2. To what limits can the technique be carried to reveal the "natural complexes" of a subject? Our data suggest that what we believed were natural complexes are disclosed especially during the first experimental sessions.[26] In any attempt to study natural com-

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[23]The general result on all cases was that four subjects showed some increase in the number of individual responses in the first hypnotic control session as compared with the first waking control session. Three of these four showed increases in the first hypnotic complex session and three showed increases in a waking complex session over the hypnotic complex session.

[24]Besides the aspects already discussed, additional analyses were made of the following: stereotypy, echolalia, individuality of response by autogeneous norms, and disturbances of the ascending and descending limbs of the voluntary responses. However, we are not reporting on these aspects in this paper.

[25]The ambiguity of the term "awareness of a conflict" must be pointed out. We find it difficult to determine what Luria means in his discussion of this point (5, pp. 149-161). There seem to be three possibilities: awareness of the presence of affect, awareness of the situation connected with affect, awareness of why the situation arouses affect. In our series the subject (*6a, 6b, 6c*) in whom "awareness" was necessary seems to favor the last possibility.

[26]Olson and Jones (6) have found that religious, political, and social attitudes may be studied by the Luria method.

plexes the complications introduced by what we have termed the "shock" effect must be considered. Both problems are probably related to personality types. Luria has suggested in another connection that there are what he calls "reactive-stable" and "reactive-labile" personalities. It is probable that natural complexes will disclose themselves more easily and shock effect be greater in the latter type.

3. Can one get information about the interrelationship of conflicts? Our material suggests that, in some subjects, the artificially induced complex may cause at least temporary disappearance of the natural conflicts, and in other subjects enhancement of the natural complexes. Also in this connection we found that the *NC* words carried fewer non-verbal disturbances after the removal of the complex than before its induction. Does the removal of one complex have a general "abreactive" effect upon the subject, or is the apparent "abreaction" due to mere repetition?

4. Can one study by this method the kinds of objects and situations to which complexes attach themselves? Witness our CA_1, CA_2, and CA_1+NC word classes. This problem is closely related to that of symbolization.

5. Are there different levels of affective discharge? If so, under what conditions does one level rather than another carry the discharge? What relationship obtains among the various discharge levels? Is there a hierarchy? Can one level become a surrogate for another? Our results indicate that the more discharge at a verbal level, the less at a non-verbal level, and vice versa. It must be remembered, however, that these differences were quite marked between the hypnotic and waking states and may be partially due to hypnosis.

6. Can a distinction be made from the pressure curves alone between affective conflicts and those which are at a more intellectual level, e.g., the attempted solution of difficult intellectual problems? Or is affect at the basis of both types? Luria has made some beginnings in this direction (5, pp. 205-239).

7. Can a complex be induced by direct- or by indirect-waking suggestion? An example of the latter might be that in which a person is made the subject of some unpleasant rumor by a group of persons.

8. What correlation would there be between sympathetic variables such as heart rate, blood pressure, galvanic skin reflex, etc. and

those studied here? In this connection it would be well to know more about gross movements of the subject which may or may not be associated with affect.

9. What are the possibilities of this method in the study of conflicts as revealed during the course of the psychoanalytic interview? It would seem that data of theoretical value could be gained from such an investigation.

10. What is the effect of fatigue and toxic states in making conflict material more available? Luria has suggested that the regulating ability of the "functional barrier" is lowered during fatigue (5, p. 384).

11. Under what conditions do perseverative effects appear? Do they tend to appear at one level more than another? There is some indication from our data that perseveration tends to appear at a level lower than that of the level of the preceding disturbance; for instance, if the disturbance is verbal then the perseverative disturbance tends to be voluntary, involuntary, or respiratory.

12. What is the effect of hypnosis on the individuality of verbal responses? (This is a question not connected directly with the Luria technique but was suggested by our experiments.) There seemed definite indications that hypnosis increased the individuality of the responses as was seen from a comparison of the means of the frequency ratings by Kent-Rosanoff standards on *N* words.[27] However, we are not certain that this is caused by hypnosis since in those subjects in whom we had a repeated waking control there was also a considerable fall in frequency. Yet there were three subjects in whom the frequency-decline from waking to hypnotic states was so marked that we feel we may be dealing with personality types in this respect. In setting up an experiment of this kind it would be necessary to take the "shock" effect on individuality of response into account.

13. What is the effect of a complex on the individuality of verbal responses? This might be studied from the point of view of Kent-Rosanoff frequencies or some autogenous standards such as a frequency table built up on a number of repetitions of the stimulus word series for each subject. There is some indication in our ma-

[27]The mean of the first waking as compared with the first hypnotic was 34.33 higher and had a Fisher's "*t*" of 4.030, a value yielding a probability of less than 0.01. This means that the difference was significant.

terial that a complex individualizes the responses on *N* words in the waking state and makes the responses less individual in the hypnotic states.

Summary

In an attempt to test the validity of the Luria method of detecting affective conflicts, one of his experiments was repeated. A complex was induced hypnotically. Verbal, voluntary, involuntary, and respiratory responses were studied. Four male and eight female subjects were used. The results obtained and the interpretations suggested are the following:

1. There was evidence that nine subjects accepted the story told them as something they had done and that it produced a profound reaction in them.

2. In six of these nine subjects some non-verbal (motor) aspect of the Luria technique revealed the presence of the conflict in either the hypnotic or waking states. These subjects in the hypnotic states tended, in general, to give verbal responses definitely related to the conflict with relatively few non-verbal disturbances. In the waking state the relative importance of the non-verbal disturbances increased over the verbal. The hypothesis is suggested that there may be "levels of discharge" so that if excitation created by the conflict is not discharged verbally there is a spread to voluntary and involuntary motor levels. An implication of this hypothesis is that the motor aspects of the Luria technique sometimes may not reveal the presence of the conflict.

3. In the three other cases, those of subjects who accepted the story suggested to them, the evidence that the Luria technique revealed the existence of a conflict was lacking or was of a dubious character. These three cases are discussed with special reference to the effect of the artificial conflict upon their verbal responses.

4. The results from the three subjects who refused to accept the complex suggest that the reproachable act must be of such a nature that the subject can plausibly conceive of his participation.

5. Data collected from repeated sessions on the same subject indicate that there is a "shock" effect which appears chiefly in the first session as a large number of motor disturbances and declines upon repetition. This "shock" effect must be evaluated properly before valid conclusions in this type of experiment can be drawn.

6. Repeated experimental sessions on the same individual while he had the conflict showed a gradual decline in motor disturbances from day to day, pointing to an "abreactive" factor or a forgetting factor.

7. Certain other theoretical implications of the experiment are discussed and a list of problems which may be approached by the Luria technique are included.

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UNE ÉTUDE DES COMPLEXES PRODUITS HYPNOTIQUEMENT AU MOYEN DE LA TECHNIQUE DE LURIA

(Résumé)

Pour tester la méthode de Luria d'associer les réponses volontaires et verbales dans le but de découvrir des conflits affectifs, on a répété l'une de ses expériences. On a fait l'essai de produire des complexes chez douze sujets pendant l'hypnose et on les a examinés avant, pendant, et après la production du complexe. On a étudié les réponses verbales, volontaires, involontaires et respiratoires, et les temps de réaction verbale. Il y a eu des témoignages que neuf sujets ont accepté le complexe comme quelque chose qu'ils ont fait et qu'il a produit une réaction chez eux. Chez six d'entre les neuf quelque aspect moteur de la technique a révélé la présence du conflit. Dans l'état hypnotique ces sujets ont tendu à donner des réponses verbales relatives au conflit avec relativement peu de désordres moteurs, mais dans l'état de réveil l'importance relative des désordres moteurs est devenue plus grande. On suggère l'hypothèse qu'il peut être des "niveaux de décharge" de sorte que, si l'excitation créée par le conflict n'est pas

verbalement déchargés, il y a une expansion aux niveaux moteurs volontaires et involontaires. Une implication de cette hypothèse est qu'il est possible que les aspects moteurs de la technique ne révèlent pas le conflit. On discute les résultats des autres sujets et d'autres implications théoriques.

HUSTON, SHAKOW, ET ERICKSON

EIN STUDIUM DER HYPNOTISCH ERREGTEN KOMPLEXE VERMITTELST DER LURIA-METHODE

(Referat)

Um die Luria-Methode, die die willkürlichen und sprachlichen Reaktionen zum Zweck der Aufdeckung affektiver Konflikte verbindet, nachzuprüfen wurde eins seiner Experimente wiederholt. Der Versuch wurde gemacht, Komplexe bei zwölf Vpn. während der Hypnose zu erregen, und sie wurden vor, während und nach der Herbeiführung des Komplexes untersucht. Sprachliche, willkürliche, unwillkürliche und Atmungsreaktionen sowie sprachliche Reaktionszeiten wurden beobachtet. Es zeigte sich, dass neun Vpn. den Komplex als etwas von ihnen Ausgeführtes annahmen, und dass er eine Reaktion bei ihnen hervorrief. Bei sechs dieser neun offenbarte irgendeine motorische Erscheinung der Methode das Vorhandensein des Konfliktes. In dem hypnotischen Zustand neigten die Vpn. sprachliche Reaktionen zu geben, die sich auf den Komplex bezogen, mit verhältnissmässig wenig motorischen Störungen, aber in dem Wachzustand nahm die relativ Bedeutung der motorischen Störungen zu. Die Hypothese wird aufgestellt, dass es vielleicht "Entladungsschichten" gibt, so dass keine Ausbreitung auf willkürliche und unwillkürliche motorische Schichten stattfindet, wenn die durch den Konflikt erzeugte Anregung nicht, sprachlich entladen wird. Eine Folgerung aus dieser Hypothese ist, dass die motorischen Erscheinungen der methode den Konflikt vielleicht nicht offenbaren. Die Ergebnisse von den übrigen Vpn. und andere theoretische Erörterungen werden gegeben.

HUSTON, SHAKOW, UND ERICKSON

RESEARCH IN PSYCHOLOGY IN COLLEGES*[1]

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ROY M. DORCUS

During the past ten years, the tendency which has prevailed in many institutions of learning and among individual psychologists to surmount the many problems of pedagogy has been to carry out more research. The motives and results of such exuberance have led many of the more conservative members of the teaching profession to adopt the panacea of "research," without any critical analysis either of its direction and ramifications or possible consequences. The approbation of such a panacea seems quite comparable to the mass approval of some programs sponsored by some of our more astute politicians. Not only have the consequences of such a program not been examined, but in addition very little attention has been given to the underlying causes of such apparent unstinted commendation. It is entirely possible that the present economic crisis may play an important part in checking this movement and give rise to some critical evaluation of the results obtained from the ambitious program already started.

Before any attempt is made to discuss the topic proper, appropriate definitions must be supplied for the terms "research," "college," and possibly "psychology." Since the majority of my auditors are psychologists or philosophers, I will assume that they have adequate knowledge for formulating a suitable definition for psychology. Even this might be questioned on a basis of some of the experiments which have been published in the psychological literature. The term "college" is one about which most professional people at least have formed some vague notion; nevertheless, if a classification of the educational institutions is made, difficulty immediately presents itself. Some institutions labeled as colleges give graduate work; others designated as universities give only undergraduate work; and certain institutions give a combination of both or even only prepara-

*Accepted for publication by Carl Murchison of the Editorial Board and received in the Editorial Office, April 4, 1934.

[1]Presidential address before the Southern Society for Philosophy and Psychology, Birmingham, 1934.

tory-school work. It is obvious that the label is no criterion. In the last analysis, the character of work given should be the deciding factor, but those of us who are familiar with the assigning of credits to students who transfer from one institution to another and who are familiar with the basic preparation of graduate students who enter the university from divers institutions realize at once that such a basis cannot be standardized adequately. For example, some colleges and universities will not allow credit to students in psychology who transfer from non-accredited institutions, that is, those institutions that are not approved in some instances by the American Association of Universities because of inadequate endowment, even though the courses may be taught by professors who are full members of the American Psychological Association.

If I were to suggest that only those institutions in which there is a staff maintained primarily for graduate work be classed as universities, then many of the institutions which have given both M.A. and Ph.D. degrees in psychology would be deleted from the classification. I do not suggest this method of classification nor do I think it desirable to adopt such a viewpoint. The words of one of the psychologists who recently wrote to me expresses indignation at such procedure. He said you would not class blank college as a college even though its name is blank college. It so happens that the college has given advanced degrees in psychology. It is not my purpose, however, to determine what institutions shall call themselves colleges and what institutions shall call themselves universities; but I am interested in the problem of research work at a certain level which may be called the "collegiate level," regardless of the name of the institution. I am also interested to a certain extent in the problem of the college versus the university in so far as research by the faculty in many of the colleges as well as the universities has become the measuring rod for promotion.

You may ask what distinction I am going to make? My reply is that I shall have to follow the accepted standards of the Association of American Universities and also rely on general information obtained from the American Psychological Association Year Book and from the literature published in the psychological journals.

The first of these agencies uses the term "college" to designate all institutions of higher education which grant non-professional bachelor's degrees. Obviously, then, practically all universities are

colleges in so far as almost all of them grant bachelor's degrees. On the contrary not all colleges give master's and doctor's degrees, but many of them do. The question is, should they be classed as colleges or universities, and should they be engaged in research work? I should like to defer further discussion of this point until I consider the problem of research itself.

What constitutes research and what standards may be applied to so-called research is a very delicate problem to analyze. Sending out questionnaires and securing information therefrom, writing up a historical survey of a topic, listening to conversation and recording the contents thereof, constructing new theories, building new types of apparatus, putting theories to scientific test, and repeating the investigations of investigators who have repeated the investigations of previous investigators are types of activity that have appeared in the literature as research. These examples of what is usually called research do not by necessity fix the limits or define the methods applied. Research may for purposes of discussion be subdivided into original investigations and other "research," including under the latter essays on topics, solutions of already-solved problems by repetition, tabulation of data from questionnaires, introspectional reports, and all forms of laboratory work. Original investigation may be defined as the attempt, or alleged attempt, to solve a problem the solution of which is, presumably, not to be found by a simple consultation of existing reports in the literature of the subject. It may be either an experimental or non-experimental approach to the solution of the problem. When most people speak of research they think in terms of the experimental approach to the solution of the problem, but in reality include all the variants which I have discussed under other "research." When I use the word "research" in my later discussions I will apply it in its conventional form, meaning of course any form of activity which one engages in and calls research. In contrast with the term "research," I shall employ the term "original investigation" and with the implication that the approach has been experimental and the rules of scientific procedure have been followed. Let us suppose that an undergraduate or even graduate student has been assigned the problem in which he duplicates the exact conditions or approximates the conditions of Ebbinghaus' original experiment on learning. Is the student doing research? According to the conception of many psychologists, and

even according to the etymology of the word, he is, but he cannot be said to be making an original investigation or even a contribution to theory or to knowledge, which brings to the fore the question as to why he is doing it at all. I should like to defer the answer to this question until I have presented some other facts concerning "research."

There are several pertinent sources of information which have been consulted which throw some light on the prevalency of research in "colleges," using the term "college" here to mean institutions which engage primarily in undergraduate work. So-called honors work, advanced degrees awarded, and publications appearing in the psychological literature are indices of research activities by both students and faculty. In the 1932 report to the American Association of University Women by McHale and Speek on "Current Changes and Experiments in Liberal Arts Education," 99 out of 172 colleges, or approximately 60%, were listed as offering honors work of some kind. This of course did not apply to all departments in all the institutions. Therefore, in order to determine the present status of honors work in psychology, inquiry was made of the 99 institutions thus reported. Information was obtained from 73 of the departments; of this number, 42, or 57%, were offering honors work in psychology; the remainder can be ignored since they offer no honors work in psychology. If the figures are viewed from another angle, I can safely say that at least 25% of the original 172 institutions listed in the American Association of University Women's report offer honors work in psychology.

The nature of the honors work was more difficult to ascertain, although in 39 of the 42 institutions it included additional reading and in 35 (chiefly the same institutions) it included work of an experimental nature. This latter work may be reasonably considered as research and in fact is usually referred to as such. Although the figures just cited are only approximately correct, they indicate the general trend in regard to so-called research in psychology at the college level since, when honors work is offered in a given institution, it is usually of a research nature and since approximately 25% of accredited institutions offer such work.

The next step following research at the undergraduate level is for colleges to undertake graduate work. While this topic is not of special interest in this paper, I should nevertheless like to present

enough information to show the general trend in this connection. An analysis of the institutions from which members of the American Psychological Association have received advanced degrees reveals the present status of psychological research in both so-called colleges and universities, since advanced degrees in psychology with the exception of honorary degrees are given almost invariably only upon the completion of a research problem, employing the term "research" in its popular sense. The number of master's degrees exclusive of those granted by foreign universities, as represented by membership in the American Psychological Association, is approximately 1200, which were granted by 107 different institutions. If each institution granted the same number, the average per institution would be about 12. Let us arbitrarily choose half this number as representative of the number of master's degrees which the average institution engaged in graduate work has granted and assume that institutions that have granted less than that number have engaged in such educational projects only as a by-product of their undergraduate work. We then find that 70 of the 107 institutions would be involved in awarding master's degrees when they are primarily undergraduate institutions. If I arbitrarily select the institutions which I know to be engaged primarily in undergraduate work, without any reference to the number of master's degrees granted, 56 of the institutions have incidentally given such degrees.

There are about 260 (this number varies from year to year) colleges and universities on the approved list of the Association of American Universities. It is obvious therefore, that about 40% of the educational institutions above the preparatory-school level are engaged in research work of some type. Of this percentage, 26 represents those institutions that give work primarily at the undergraduate level.

The analysis of institutions awarding Ph.D. degrees, while interesting, does not shed much light on the problem of research in colleges. The 1100 Doctor of Philosophy degrees represented by membership in the American Psychological Association were granted by 46 institutions. This means that approximately 25 degrees represent the average for each institution. If I again select half this number as representative of the number of degrees an institution primarily engaged in this work should have granted, 27 of the institutions would be eliminated; or, stated in another way, 60% of

the institutions which have granted Ph.D. degrees are doing so as a by-product of so-called research instituted at the college level.

You may contend that the criteria of research in colleges which have been presented are invalid since the assumption has been made that advanced degrees and honors work in general indicate research work. If this contention should be admitted for the sake of argument, there remains another criterion that cannot be ignored, namely publications. That is, unless the further admission is made that publications are not evidence of research, and with that assumption I am partially in agreement. Certainly some publications are not research and a still much greater number are not original investigations.

The articles which have been published during the period from 1928 to 1933 in *The Psychological Bulletin, The Psychological Review, The Journal of Applied Psychology, The Journal of General Psychology, The American Journal of Psychology, The Journal of Experimental Psychology* and *The Journal of Comparative Psychology* number 1504, exclusive of notes, discussions, articles on apparatus, and reports of meetings. Various reports have been made in which an analysis of the trends in psychological research have been studied. My purpose is not to show the trend in psychological experimentation but only the prevalence of research in colleges. I am cognizant of the fact that the literature does not give an adequate picture of the so-called research done by undergraduate students since much of the work is filed away on departmental shelves and some of it is later amplified and published under the names of the professors. Just how widespread this practice is cannot be ascertained and for the good reputation of the profession it may not be desirable to ascertain it. Articles published under the joint authorship of faculty and students represent only a portion of the total.

The 1500 articles in the seven sources mentioned show that 72 institutions that have no graduate department or are definitely labeled college have contributed. Of these 72 institutions from which publications have appeared, 11 of them have given advanced degrees in spite of the fact that they have no definite graduate school and are called colleges. The contribution of the 72 undergraduate institutions has been 195 publications or 13% of the total. While the majority of the colleges are credited with only one publication

each, the major number are contributed by about six colleges in which the system of faculty-student publication is in vogue. It is not implied that the faculty in these colleges do not publish some papers alone. The frequency of the practice is indicated by the fact that 16% of the publications from colleges are joint faculty-student publications.

The reports from the psychology departments offering honors in the undergraduate school show that 19 of the 41 departments publish the results of investigations in some form, while the remainder signify that publication is not attempted. This means, of course, that 50% of the institutions now publish and many of the others expressed the desire for publication if possible. Since articles are accepted by most journals only if they are sponsored by a member of the teaching staff, an increase in the number of student-faculty publications may be expected. This hypothesis is based on the statements made by psychologists in colleges who informed me that they desire to start publication of student research as soon as feasible. While a critical evaluation of the articles published would be desirable, no such procedure is possible. The merit of publication is presumably passed on by the editors of the respective journals, and, while I might personally disagree with some of their decisions, I shall not set myself up as czar of psychological research. It would nevertheless be interesting to have an unbiased committee pass upon the value of such publications as contributions to psychology. I might point out, however, that two of the journals examined contained by far the greatest number of articles from colleges.

What the editorial policy of these journals is cannot be stated. Either the type of material which is ordinarily published in those journals lends itself for experimentation in colleges or else the probability of getting submitted articles published is greater. I know that the cost of publication does not enter, since I am familiar with the relative cost of publication in the various journals.

The alleged reasons for carrying on undergraduate research are (1) interest in furthering psychological knowledge and (2) training students to attack new problems. To these alleged motives I should like to add (3) promotion of faculty members either through the number of publications or through the number of majors secured for the department, (4) increasing the number of graduate students which in turn reflects on the prestige of the professor, and (5) the

advertising of the college through publication and number of students presumably entering the graduate field.

Whether the reasons operate singly or as a group cannot be determined except in individual cases and even then the *modus operandi* has to be inferred. The first of these may produce one of two or both of the following results: an increase of psychological knowledge in general or a specific increase of the students' knowledge. While I presume the second reason is interwoven with the first, some psychologists contend frankly that research is undertaken primarily to train the student in logical thinking. They admit that the student does not acquire any specific psychological knowledge that could not be acquired with a much less expenditure of both time and energy on the part of the student and the staff. The problem is also complicated by the nature of the research undertaken. If the problem assigned or attacked falls into the category of an original investigation, the question is pertinent whether the student is capable of solving it in view of his limited preparation in psychology as well as the other fundamental sciences. In general, the answer must be in the negative. Most graduate students are not prepared for undertaking original investigations in their first year. They are not prepared to recognize an original problem and if one were assigned by the professor their limited knowledge of the literature and apparatus is an insurmountable handicap. Many graduate departments require preparation in physics, biology, neurology, mathematics, logic, and chemistry before graduate students are allowed to undertake original investigations. If this preparation is fundamental then certainly undergraduates can only be thrown into a state of confusion when confronted by a task which requires such basic preparation.

If the students are allowed to undertake research in contrast with original investigations, do they accomplish the objective of the alleged reasons for doing research and could the objectives be attained in a better manner? If the problem is one of repeating research, no contribution is made to knowledge, nor is the student doing any creative thinking. In all probability the laboratory facilities will not even permit an exact duplication of the research, therefore if some differences in results are obtained it only arouses skepticism of the validity of all psychological investigation on the part of the student.

I am not convinced that the primary reason why undergraduate students are interested in the tedious tasks of observing an animal

run a maze daily, of learning tactual localization, of observing the effect of noise on work, and of analysing the different degrees of color saturation is love of knowledge. Many of these students admit to other professors and to other students that the problems are endured to secure marks. In some cases, enthusiasm is aroused by the instructor by creating a false impression of the value of the work. Since the student is not in a position to judge, and since so-called research is so heartily commended when done by the staff, the natural feeling would be one of enthusiasm. But uncritical enthusiasm can be deflated quickly and the end result may prove disastrous to some disillusioned students.

If the work is undertaken to give training in procedure and method, this can be done more efficiently and economically by a laboratory course in which the apparatus and technique is standardized for the problem. Demonstrations prepared by the instructor would be more efficient for illustrating and emphasizing the points in question. If the objective is logical thinking, then it seems to me that we are indicting courses in logic or else we should admit that the proper teachers of logic are logicians.

Some advocates of research emphasize the influence of the training upon the solution of the problems which arise later in life. If the problem is an economic one, it could be solved more adequately by training in economics; if it is one of physical hygiene, work in physiology would be more appropriate; and if it is one in domestic science, a course in cooking would be more beneficial than additional research in psychology. I do not wish to convey the impression that work in psychology should not be offered in generous amounts, but I am questioning the desirability of undergraduates devoting the time to research which it ordinarily requires. It is not a question of training specialists but is gradually crystallizing into a problem of that kind.

The last three reasons for engaging in research are of primary concern to the teaching staff and administrative officers of the college. The administrative powers are either directly or indirectly responsible for encouraging research. Observing instructors have looked at the advancement of their colleagues and in casting about for an explanation have found that the essential element in advancement has been publication and activity in the professional organizations. Too often the administration has overlooked "teachers,"

and the end result has been to instigate competition in research even at the expense of impairing the more fundamental activities. Individuals have told me privately that administrative officers of their colleges have definitely stated that research would be looked upon with favor and even in the absence of such a direct statement the policy of a given college can be readily determined.

The appropriations of most departments are determined by the number of students enrolled in the classes or the number of students majoring in a department. In face of existing conditions, the usual reaction is to develop courses that tend to increase enrollment and in turn increase appropriations. Upon superficial examination, research by students would not tend to increase enrollment in courses. This is partially true, but research is presumably a by-product of class work in some institutions, and in others it is not. Now if we consider the first method of conducting it, the professor has the opportunity of approaching the administration with the plea that another instructor is needed since he has a number of sudents so vitally interested in the subject that additional time must be given to them. If the second method of handling undergraduate research is employed, a complicated system of evaluating numbers of students is often introduced, so that the direction of one student doing research is the equivalent of teaching any number of students a three-hour course.

Although you may disagree with my statements, additional evidence is to be had which cannot be overlooked. Membership in the professional and honor organizations is awarded only to those individuals who have done research work.

The American Psychological Association imposes as a qualification for membership the following regulations: "The conditions for Membership shall not be considered as having been fulfilled in the absence of (1) acceptable published research of a psychological character beyond the doctorate dissertation and (2) of the degree of Doctor of Philosophy, based in part upon a psychological dissertation." Membership in the Southern Society for Philosophy and Psychology is not dependent upon research, although one of the aims as stated in the constitution is to promote original investigation among its members. Any one, then, who enters the teaching field of psychology is almost automatically forced to engage in research to gain promotion, to secure recognition in the national and sectional

organizations, and even to be considered a suitable member for such organizations. In the presence of such regulations imposed on the members of the profession, can it be said that zealousness of research alone is the impelling force in such activity?

Faculty advancement cannot be entirely separated from promotion of the college because of a number of factors common to both forms of activity. Faculty members are judged indirectly by the number of students who go on for advanced degrees either in the same institution or in other institutions. The judgment passed is of a twofold nature, i.e., judgment of proficiency by colleagues in the profession who later train the students and judgment by the college administrative officers. The question is seldom raised whether an increase in the number of graduate students is desirable. Evidence that administrators look with favor upon these activities is abundant. In accrediting colleges, one criterion of eligibility is expressed by the following section taken from the report of the American Council on Education. The statement reads, "In determining the standing of a college, emphasis should be placed upon . . . . and its success in stimulating and preparing students to do satisfactory work in recognized graduate, professional or research institutions."

I should like to call attention particularly to the phrases "stimulating students to do satisfactory work in graduate or research institutions." In view of this avowed objective, we may wonder why research is not stressed even more than it is at the present time. Collections of faculty reprints and publications are maintained by many colleges and the individual professor cannot ignore the challenge to his ability by not having his name appear in the group. Not only can he not afford to have his name missing but he cannot afford to forego the publicity of an intramural nature or in the newspapers incident to publication of most kinds. In the long run, this reflects on student approbation. Similarly, some college presidents are not averse to having the activities of the college appear in press. Although publicity of this kind is legitimate, it is unfortunate that in some instances those members who engage in research work dominate the affairs of the college thereby and in turn instigate competition in research.

In spite of the platitudes of some speakers who extol research in colleges, the practical situation of whether colleges can afford it cannot be ignored. These speakers discuss this aspect of the situa-

tion by referring to the cost of maintaining armies and navies and by pointing out other forms of activity supported by the public treasury. They are of course only stating the relative values of such projects in terms of what they feel is the importance of college research. While this line of reasoning may sound plausible, the fallacies are only too obvious. It is exceedingly difficult if not impossible to determine the cost of research, but some estimates of the relative cost of graduate versus undergraduate instruction in psychology obtained from five representative universities throw some light on the problem. These institutions will not be identified for reasons which are pertinent to the individual institutions. I feel that the inferences concerning cost of research that may be made from such a comparison are valid because a large portion of the training in graduate work is of a research nature.

The relative costs are as follows:

Institution	Graduate instruction	Undergraduate instruction
1.	$ 67.43 per point	$ 13.32 per point
2.	$2100.00 per year of 3 quarters	$540.00 per year of 3 quarters
3.	$ 110.00 per course hour	$ 11.90 per course hour
4.	$1000.00 per student per semester	$ 20.00 per student per semester
5.	$ 18.00 per student per clock hour	$ 18.00 per student per clock hour.

The different methods of computation are somewhat confusing, but if these figures are reduced to ratios the range is from 1:1 to 1:50; of these, two institutions, the fourth and fifth, represent the extremes. The other three institutions are more nearly in agreement; the ratios being 5:1, 4:1; and 10:1. There are special reasons why the ratio for institution number 5 is of the order 1:1. Under any circumstances, if a college is to undertake training essentially like that fostered in the university it must be prepared to expend from four to five times the amount of money it is at present outlaying for instruction.

The figures which have been presented only indirectly reflect the cost of an original investigation. Any single project may mean the outlay of from a hundred-dollar minimum up to thousands of dollars depending upon the apparatus which is already available and the nature of the project. In an attempt to arrive at costs involved in research or rather investigations of an experimental nature, the

grants-in-aid rendered by the National Research Council show at least a portion of such costs. Since its inception thirty-three grants have been made to psychologists. This number should be reduced by four since these represent additional grants for extending the same project. Of this number, only two grants have been made to workers in institutions that are strictly of the college level. One of three conditions must exist: that psychologists in colleges have not applied that the projects submitted were not considered as worthy of aid, or that the committee from their experience with scholars value university candidates more highly.

The average grant for the 29 projects was $584.00 with a range from $100.00 to $1500.00. If we assume that these costs are representative costs of experimental investigations, and I believe that the assumption is valid, then each such investigation will cost the college not only the above amount but also additional money for purchasing apparatus which is not available in most college laboratories. We are forced to the conclusion in the face of the evidence available that colleges are not in a position financially to undertake experimental investigations. Whether they may undertake research of a different character is another question and it is also another question as to whether these latter types of activity should be called research.

The cost of collegiate research does not stop with the actual cost of the apparatus, but additional debits must be listed as a result of the time consumed by the projects. The Association of American Universities states that teaching schedules exceeding 16 hours per week per instructor should be interpreted as endangering educational efficiency. If this is an accepted fact then the college instructor who engages in research or supervises research can be charged with educational inefficiency. These instructors in colleges are rarely if ever relieved of the normal teaching load. It has been maintained that the instructor does such work on his own time. I contend that such time is not available when a teaching load of 16 hours is carried. If research by the students is counted as class work, then each student must cost the college some where in the neighborhood of $400.00 or $500.00 in terms of time devoted by the instructor.

The actual outcome of research at the college level is an overproduction of graduate students, an increase in the literature for the already over-burdened psychological journals, and an overloading of the staff. While I am not advocating an abolition of the quest

for knowledge in colleges, I do feel that less emphasis should be placed upon research, so that those individuals who do not care to undertake it shall not be forced into it unwillingly. Perhaps some system could be worked out whereby the college instructors who wish to engage in research and thereby keep in contact with scientific advancement could spend some time during the summer in universities where facilities for research are available and where routine courses are not given.

AN EXPERIMENTAL STUDY OF THE MOTOR THEORY OF CONSCIOUSNESS: I. CRITIQUE OF EARLIER STUDIES*

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The central principle of the motor theories of consciousness is that all awareness is essentially dependent upon motor response; that consciousness is not a correlate of merely cortical activity, but is intrinsically a reaction, functioning in terms of complete sensori-motor arcs, with the motor part of each arc just as essential to the process as the central segment. The specific motor theories which are of present concern are those of (1) Dunlap, (2) Washburn, (3) Watson.

Historical

Certain linguistic phases of the motor theory have been traced by Thorson to Bain and Ribot (1879). While Munsterberg's "action theory" (1900) possessed some elements of a reaction basis of consciousness, the motor theory in its modern form seems to have originated with Dunlap.

1. Dunlap's reaction hypothesis dates back to 1907 (5, 7), when he began to urge the abandonment of the conventional theory of the mental image as a centrally aroused sensation, possessing the same modality as the original sensation from which it derived. He proposed instead the hypothesis that the sensory tang which seems to characterize an "image" of given modality originates not in the sense organ itself but in the motor apparatus functionally connected therewith. As he put it later,

> In thinking of a visual object, e.g., of an illuminated sign, there are movements of accommodation and convergence of the eyes, if the person is of the "visual" type. In thinking of the sound of an orchestra there are changes in tension of the muscles of the tympanum of the ear, or in the neck muscles (1912).

*Accepted for publication by Knight Dunlap of the Editorial Board and received in the Editorial Office, February 15, 1934.

Thus conceived the image "renders an explanatory service badly needed in psychology"; for its muscular aspect provides the missing link in the physiological mechanism underlying associative thinking.

Dunlap deduces the physiological basis of both thought and perception from the reaction-arc hypothesis itself, and applies the principles of habit formation not only to habits of action but also to habits of perception and thought.

> Consciousness, whether perceptual or ideational, depends upon reaction, which is primarily a discharge from receptors, through the central nervous system, to effectors, and terminating in specific activity of those effectors. The route over which the discharge travels is designated a reaction arc. . . . No consciousness occurs without a complete reaction. . . . Having regard to the termini of the arcs, we can distinguish three kinds of reactions: striped-muscular, smooth-muscular, and glandular. . . . Arcs which connect similar structures . . . . may be called *homeodetic,* and those which connect dissimilar structures . . . . *heterodetic.* Heterodetic arcs may terminate in any of the three classes of effectors; homeodetic arcs are perhaps of the muscular types only, since there have been no afferent terminals discovered in glands. . . . Since the muscle-spindles which lie in the striped muscles are excited by each contraction, . . . the peculiarity of the homeodetic reaction is that the effect of one reaction initiates another. The heterodetic striped-muscle arc conditions perception, and the homeodetic arc conditions thought.

As a supplementary hypothesis, Dunlap assumes that

> The reactions are ultimately abbreviated or short-circuited. . . . The complete muscular response is necessary during the learning process, but is largely eliminated, in the interests of economy, after the series has been thoroughly mechanized. The known functions of the cerebellum point to it as the possible short-circuiting mechanism (6).

2. In September 1914, Washburn presented a theory of thinking similar to that which Dunlap had described the previous March. She, too, invoked the "established phenomena of habit formation."

> To that form of association which has been traditionally called the association of ideas, the association of movements is preliminary.

> Now the most natural way in which we may suppose such movement systems to be learned is by some arrangement through which the performance of one movement may itself furnish the stimulus, or part of the stimulus, for another movement. And the most obvious method by which the performance of one movement may regularly provide the stimulus for another is by the processes which are set up in sensory pathways by the action of the muscles themselves.

This thesis is further elaborated in *Movement and Mental Imagery* (1916) in which she contends that "consciousness is accompanied by bodily movement" and

> utilizes, as actual causal mechanisms, certain motor processes which have been neglected by psychological theory as mere incidental phenomena. . . . The most important of these neglected motor processes are the slight actual muscular contractions (tentative movements) which accompany all attentive consciousness and are the basis, I believe, of all associative activity,—all association being merely association between movements. . . .
>
> Such tentative movements are very economical of energy. They involve far less fatigue than full movements, and can be performed far more rapidly . . . . they are actual slight contractions of the muscles which the larger movements would involve; they are sometimes discoverable by introspection, sometimes not. . . . Finally, it would seem highly probable that the cortex is the organ for tentative movements, and that on the development of the cortex the development of tentative movements rests.

3. Watson's theory of thinking has been extensively popularized and does not require exposition here. The common impression that "Watson was the first to maintain the identity of thought and language activity" is not strictly true, for F. Max Mueller seems to have anticipated him by twenty-five years. In 1887, Mueller wrote, "Our divine reason is really no more than human language." In one of his lectures before the Royal Society on "The Identity of Language and Thought," he said: "No reason without language, no language without words." "We do not begin with thinking or ideation and then proceed to speaking, but we begin with naming." "We can as little think without words as we can breathe without lungs."

Watson's doctrine of implicit speech is also foreshadowed.

> Reasoning without words is no more than reasoning without pronouncing words. . . . It is the highest excellence of language that it can thus abbreviate itself more and more. If we had to pronounce every word we are thinking, our progress would be extremely slow. . . . And yet, if we watch ourselves, it is curious that we can often feel the vocal chords and the muscles of the mouth moving as if we were speaking. . . . I still hold that it would be difficult to invent a better expression for thinking than that of the lowest barbarians, *"speaking in the stomach."*

Mueller's thesis was strongly contested, and elicited a rather lively correspondence in *Nature* (1887) on "Thought Without Words." This discussion, participated in by Galton, Romanes, the Duke of Argyll and others, bears striking resemblances to the 1920 Symposium on "Is Thinking Merely the Action of Language Mechanisms?" (20) and to other criticisms (13) which Watson's theory evoked.

From a chronological point of view, therefore, Watson's laryngeal theory is to a large extent the old wine of Mueller put into the new bottles of a stimulus-response terminology. And the stimulus-response aspect of the behavioristic explanation of thinking had in turn already been anticipated by Dunlap's reaction-arc theory (5, 21). Watson's contribution seems to be largely one of emphasis: Neither Mueller nor his contemporaries thought of submitting the issues involved to experimental investigation—a possibility which was considerably stressed in Watson's formulation. Watson's more recent revision of his thought theory (1925) adds certain features similar to the formulations of Dunlap and Washburn.

Earlier Investigations

The results of several early experimental studies have a more or less direct bearing on the motor theory, particularly those of Hansen and Lehmann (8), Jastrow (9), Tucker (16), Curtis (3), Courten (2), Wyczoikowska (23), Reed (1916), Clark (1), and more recently Thorson (15). Since most of this work has already been abstracted in the last mentioned paper, it need not be reviewed here.

The positive results of Courten and Wyczoikowska probably cannot be accepted without question. Courten recorded "Involuntary

Movements of the Tongue" during thinking by means of a rubber-bulb, which was placed in the front part of the mouth, between the teeth and the tip of the tongue, and was connected to a recording tambour. "No record was taken which did not show some movement: unconscious movements of the tongue do take place during mental action." Wyczoikowska employed "a sort of flattened wine-glass as a receiver, a Marey tambour with a longish pen, and a Ludwig Kymograph. The glass, which was large enough to admit the end of the tongue without impeding its movements, was prolonged as a tube, which was connected to the tambour by a rubber tube." Her conclusion was that "every thought produced some movement of the tongue." But Courten's rubber-bulb would be affected, as Thorson points out, by respiration and swallowing movements; and Wyczoikowska's glass capsule, while not of the rubber-bulb type, would probably be similarly affected. The tracings of these two workers, therefore, may have been of partly adventitious origin, thus invalidating their positive conclusions.

As to the negative conclusions of Reed, Clark and Thorson, those too cannot be regarded as having been experimentally established. Reed concluded that while for some subjects thinking was definitely a process of tongue movements, this is purely an individual trait and not a universal one. Clark similarly concludes that while "inner speech plays an important part in thought," "the greater part of thinking is carried on in terms other than inner speech." And Thorson likewise: "Movements of the tongue are not universal in internal speech or verbal thought. . . . This leaves only the hypothesis that the activities are intra-neural, and do not necessarily involve complete motor expression at each stage of the process."

So sweeping a negative conclusion immediately evokes a question as to the adequacy of the apparatus employed. Though different investigators used different types of capsules to receive the movements of tongue and larynx, all of them employed some form of lever arrangement to record these movements: Courten, Curtis, Pintner, Wyczoikowska, Reed, and Clark used tambours for this purpose, while Thorson employed a double lever-system, a modification of Somner's tridimensional analyzer specially designed by Lashley for this experiment. Were such lever arrangements sufficiently well adapted to record all movements that may have occurred? I do not believe that they were, because, even if we assume that the magnifica-

tion was adequate, the requirements as to sensitivity and natural frequency were not met.[1]

The magnifications of the tambours are not stated, but Lashley's levers had magnifications of 3.5 and 4.5 respectively; "therefore [it is claimed] an actual tongue movement of 1/4 or 1/5 of a millimeter could easily be recorded." The question immediately arises, however, was the apparatus of sufficient sensitivity to record every movement of 1/4 or 1/5 of a millimeter? No information is given on this point but I do not think that it was. For the tongue might actually move a fraction of a millimeter when unencumbered, yet the energy behind this movement might not be sufficient to activate a lever system. A lever will respond differentially to movements of different intensities only when the energy of each of the movements is above a certain minimum; at or below that minimum the response is an all-or-none affair. Every lever requires a given amount of energy to overcome its inertia and any force below this "threshold" value, even if it is a small fraction of a dyne below it, will not be able to budge the lever in question.

From a study of the diagram of Lashley's analyzer it appears that the design is far from satisfactory from the viewpoint of sensitivity, since the thumb-screws, attached as they are to the levers themselves, considerably increase the inertia of the moving system. Thus, since each tongue movement had to overcome the inertia not only of one but of two levers, plus the friction of these against the smoked paper, this device probably did not succeed in registering all the movements made by the subject. There were undoubtedly some which were too feeble to activate the levers.[2]

Its insufficient sensitivity might also explain why this analyzer failed to show a qualitative resemblance between tongue movements of overt and implicit speech. Since the muscles involved in overt speech do not all contract with the same degree of force, at least some of these intensity gradients would, by hypothesis, be retained in the implicit miniatures of the overt movements. In the complete overt movement most of the less intense components would probably be above the minimum necessary to move the levers. In the implicit miniature, however, many of these feebler components would fall

[1]The effect of the damping factor is not here treated.

[2]For suggestions and design characteristics for a highly sensitive type of lever arrangement, cf. the present writer's torsion myograph (1931).

below the lever's threshold and the resultant curve might look considerably different. This point is illustrated later.

But more important even than sensitivity is the question of the natural vibration-frequency of the recording apparatus as compared with the frequency of the phenomena to be recorded. Physically, a tambour is simply a compound pendulum possessing a vibration-period which is inherent in the system and which prevents it from recording faithfully phenomena of a higher frequency. It is important, therefore, to know how the frequency characteristics of the tambours compare with those that might be present in the implicit movements.

In the absence of accurate information on the maximum frequency of implicit movements, perhaps empirical observation may be resorted to. I can think from *a* to *l* in the alphabet during a second's interval; certainly I can think the phrase "physiological psychology" in a single second. Theoretically, then, we may make as many as 10 or 12 implicit movements per second, and any recording lever or tambour must have an inherent frequency of at least that—ideally, twice that. Since none of our investigators has measured the frequency characteristics of the apparatus used one must resort to conjecture. In the Mackenzie polygraph (1926 model), in which the tambours are decidedly more delicate than the "inscribing tambours of the ordinary Marey pattern" used by the above investigators, the membranes plus the writing-levers have an inherent vibration-frequency of only 4.5 per second. It seems reasonable to infer that the tambours under discussion were no faster than 4.5 per second and were therefore incapable of giving a faithful picture of any but the slower implicit movements. In Clark's experiment, the laryngeal movements had not only to activate a tambour membrane with its lever, but in addition had to move the whole of another receiving tambour;[3] thus an inherent frequency of 4.5 per second is really a generous estimate for her apparatus.

What has here been said of tambours probably applies also to Lashley's analyzer: in the latter a given movement had to activate two levers with their attached thumb screws, in the tambour one lever plus an elastic membrane—conditions sufficiently akin to make the above observations applicable to both. This insufficient frequency of the analyzer may further help to account for Thorson's

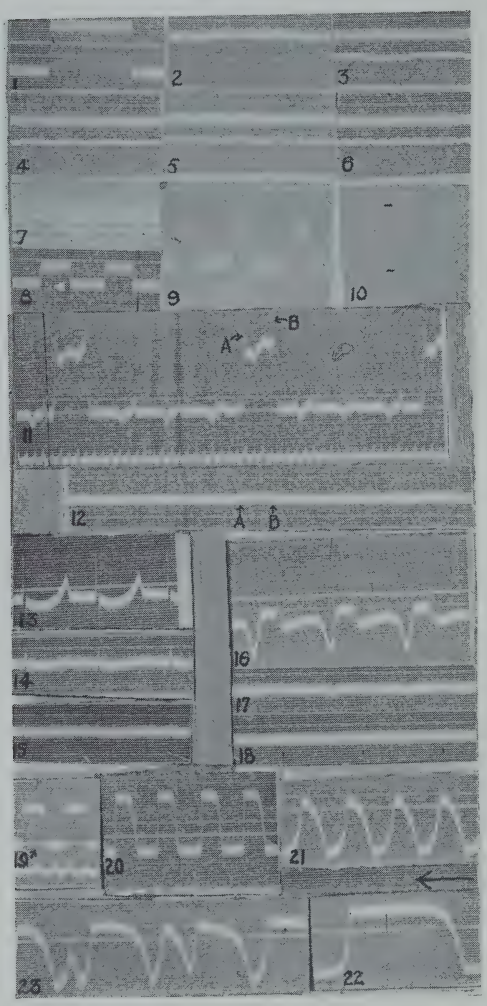
[3]Her Figure 6, p. 64.

failure to find a greater resemblance between her overt and implicit curves. No picture of movements faster than the analyzer itself would be accurate, since the curves would tend to be distorted; and this holds even for overt movements. Since the particular form which the distortion would take would vary with the rate of the impressed movements, whispering a phrase at a different rate than "thinking" it, might easily give a different curve—as might also repeating the same verbal thought at different rates. Such variations in the rates of making the movements in question are probably the rule rather than the exception: one can think the letters from *a* to *k* much faster than one can whisper them; and where (as in the experiments under discussion) speed of response was not controlled, a subject might whisper or "think" the same group of syllables more rapidly at one time than at another. Inspection of Thorson's negative kymograms, had any of them been published, would immediately show how far the foregoing observations apply. But even the three positive curves published give evidence of distortion due to the frequency and sensitivity factors under discussion.

Comparison with the String Galvanometer

The foregoing criticisms apply not only to relatively heavy levers and tambours, but even to as delicate a moving-system as the microscopic string of the Einthoven galvanometer. This is demonstrated by the curves in Figures 1 to 23, which will also illustrate concretely the operation of the sensitivity and frequency factors as above discussed.

Although the string galvanometer is an electrical instrument, the mechanics of its action is directly analogous to that of the tambour and lever systems. For the string-galvanometer consists of a powerful electromagnet the poles of which are very close together, and a finely drawn conducting fiber or "string," stretched vertically in the narrow gap between the two magnetic poles. By passing an electric current through this string, we force the string to move sidewise, i.e., in a plane at right angles to the lines of magnetic force across the gap; in other words, this is merely a convenient way of impression a mechanical force upon the fiber. In the following curves the galvanometer was maintained at fixed sensitivity and period; by passing known currents through it mechanical forces of varying intensities were applied.



FIGURES 1-23

Figures 1-18 illustrate by means of string galvanometer the effects of inadequate sensitivity. *Quantitative Effects:* Figures 1-10. Figure 1, mechanical response to impressed current of 2.4 x 10^{-6} ampere. Figure 2, response to 1 x 10^{-7} amp. Figure 3, 1 x 10 amp.: a minute response just discernible, accompanying downward deflection on signal (lower) line. Figure 4, current of 5 x 10^{-9} amp., acting during downward deflection of signal marker; Figure 5, 1 x 10^{-9} amp.; Figure 6, 3 x 10^{-10} amp.; no

visible effects. *Effect of Magnification*: Figures 7-10. With a subliminal force (3×10^{-10} amp.), doubling the optical magnification to 1800x in Figure 7 is ineffective, as was also an increase to 3600x. With a superliminal force, the deflection shown in Figure 8 with a magnification of 900x is doubled in Figure 9 at 1800x and quadrupled in Figure 10 at 3600x.

Qualitative distortions, due to lack of sensitivity: Figures 11-18. Figure 11, electrogram from injured frog's heart. Figure 12, same wave form recorded at decreased sensitivity: all the finer details are lacking. Figures 13-15, records of another frog-electrogram at progressively decreased sensitivities; Figures 16-18, ditto; the qualitative resemblance decreases directly with the sensitivity.

Figures 19-23: *distortion due to inadequate frequency of recorder.* (Curves to be read from right to left). A square-topped rectangular wave-form, of type shown in Figure 19, was impressed on strings of progressively slower periods in Figures 20-23. Both qualitative (Figures 20-22) and quantitative (Figure 23) distortion resulted.

Figure 1 shows the movement of the string to an impressed current of 2.4 microamperes. Figure 2 shows the response to approximately 1/10 microampere—small but definitely present. In Figure 3, 1/100 microampere was introduced at the indicated intervals on the signal line. No response is here visible to the naked eye; but with a good reading-glass a deflection of less than one tenth mm. is just discernible in the original record. We may regard this as the "threshold" response. In Figure 4 the impressed current was decreased to one-half the last value, in Figure 5 to one-tenth, and in Figure 6 to one-thirtieth. None of these produced any visible effect. Thus, even the microscopic fiber here acts in all-or-none fashion; once the current is below its threshold value, the string does not respond at all.

Nor is magnification of any avail at these subliminal levels. In Figure 7 the same force which is exerted in Figure 6 is repeated with double the string magnification; the string still fails to respond. The same was true with quadrupled magnification. Once one returns to superliminal values, however, magnification is an effective aid: the same current which gives a response of about 6 mms. in Figure 8 yields 12 mms. in 9, and 24 mms. in 10, taken at double and four times the optical magnification, respectively.

The qualitative distortion resulting from insufficient sensitivity is illustrated by Figures 11 to 18. In Figures 11 and 12 the same wave-form was photographed at progressively decreased sensitivities. (This voltage-wave is an electrogram from an excised frog's heart, so injured as to yield a maximally irregular curve.) The peak B

in Figure 11 continued beyond the edge of the film and measures more than 12 cms. With the decreased sensitivity used in Figure 12 all the finer details of this electrogram have disappeared, only deflections B and A remaining. In Figures 13 to 15, similarly, the qualitative resemblance between the three photographs of another wave-form (frog-electrogram) clearly decreases with the sensitivity. The same is true in Figures 16-18. This effect was likewise obtained in the converse situation when the sensitivity was kept constant and the impressed voltage was progressively attenuated.

Turning now to the frequency factor, the actual effect of too low a natural frequency is demonstrated by Figures 19 to 23. A rectangular square-topped wave, of the shape shown in Figures 1, 8, and 19, is impressed upon strings of progressively slower natural periods.

When the inherent vibration-rate of the string is slower than the frequency of the wave-form, distortion results. In Curve 20, e.g., where the galvanometer period is slightly slower than the wave-frequency, the rectangular appearance begins to change; and this effect becomes even more marked in Figure 21, where the galvanometer is slower still. The manner in which this distortion arises will be better understood from a study of Figure 22, which is a record of a single rectangular wave. Had the galvanometer period been adequate, only perpendicular lines and right angles would have been recorded: the curved lines are purely artifacts. The distortion is most evident when the waves are not of a periodically repetitive nature, but are irregularly spaced as in Figure 23. Here the five downward waves, though actually right-angled at both top and bottom as in Figure 19 and all exactly equal in amplitude, are distorted beyond recognition.

These galvanometric records illustrate clearly the type of inaccuracies that may be introduced by the physical limitations of the apparatus employed. If such limitations characterize even the string galvanometer, which has a moving element weighing only about 0.003 milligram and which employs inertialess (optical) means of magnification, they apply with much greater force to the relatively ponderable lever systems earlier employed. It is these limitations which may account for the negative findings, both quantitative and qualitative, of the above-mentioned investigations. A definitive test of the motor theory, along the lines initiated by these particular

experimenters, would therefore require more adequate types of recording apparatus. For this reason, the present writer has turned to the electrical method of registration,[4] a detailed description of which will follow.

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UNE ÉTUDE EXPERIMENTALE DE LA THÉORIE MOTRICE DE LA CONSCIENCE. I. HISTOIRE ET CRITIQUE

(Résumé)

On donne un résumé des contributions respectives de Dunlap, Washburn et Watson à la théorie motrice de la conscience, et on fait une évaluation des études expérimentales antérieures où il s'agit de cette théorie. Les conclusions positives de Courten et de Wyczoikowska sont inacceptables à cause de la possibilité que leurs tracements ont été en partie de l'origine adventice. On ne peut pas considérer que les conclusions négatives de la pluparts des autres étudiants sont expérimentalement établies, puisque l'appareil d'enregistrement qu'ils ont employé a été (1) d'une sensitivité insuffisante, et (2) d'une fréquence de vibration trop peu élevée pour la fréquence des mouvements à enregistrer: les mouvements implicites supposés par la théorie motrice peuvent avoir lieu à deux ou trois fois la vitesse à laquelle les tambours ou les leviers ont été capables de marcher. Les défectuosités graphiques causées par une sensitivité insuffisante et une période insuffisante sont illustrées d'une manière concrète au moyen de quelques enregistrements faits par le galvanomètre à ficelle. C'est à telles défectuosités et inexactitudes que l'auteur attribue les résultats négatifs, et quantitatifs et qualitatifs, des études discutées. On considère plus suffisante la méthode électrique de l'enregistrement.

Max

EINE EXPERIMENTELLE UNTERSUCHUNG DER MOTORTHEORIE DES BEWUSSTSEINS. I. GESCHICHTE UND KRITIK

(Referat)

Die einzelnen Beiträge von Dunlap, Washburn, und Watson zu der Motortheorie des Bewusstseins werden aufgerollt, und eine Abschätzung der früheren experimentellen Studien über diese Theorie wird gemacht. Die positiven Schlüsse von Courten und Wyczoikowska sind unannehmbar wegen der Möglichkeit, dass ihre Zeichnungen teils zufälligen Ursprungs wären. Die negativen Schlüsse der meisten anderen Forscher dürfen nicht als experimentell nachgewiesen angesehen werden, da die Apparate, die sie angewandt haben, (1) von einer unzulänglichen Empfindlichkeit waren, und (2) von einer ungenügend hohen Vibrationshäufigkeit im Vergleich

zu der Häufigkeit der aufzuzeichnenden Bewegungen: die impliziten Bewegungen, die von der Motortheorie vorausgesetzt werden, können zwei- oder dreimal schneller stattfinden als die Geschwindigkeit, bei der sich die Tamboure oder Hebel zu bewegen vermochten. Die graphischen Entstellungen, die durch die unzulängliche Empfindlichkeit und den unzulänglichen Zeitraum verursacht wurden, werden vermittelst der Aufzeichnungen von dem Schnurgalvanometer sachlich illustriert. Der Autor schreibt den negativen Befunden der oben besprochenen Versuche diese Entstellungen und Ungenauigkeiten zu, sowohl die quantitativen wie die qualitativen. Die elektrische Methode der Aufzeichnung wird als zuverlässiger betrachtet.

MAX

UNITARY ABILITIES*

From the Department of Psychology of The University of Chicago

L. L. THURSTONE

The purpose of this note is to describe a method of isolating unitary abilities. I discovered the method several weeks after the Chicago meeting of The American Psychological Association in September, 1933. This method of isolating unitary abilities is included in a theoretical volume which is in preparation but the early publication of the method may be of use to the increasing number of investigators who are using factorial methods. The method has been devised especially for test data but it may be applicable also to other types of variables.

It will be assumed here that a factorial matrix F has been obtained such that the matrix multiplication $FF' = R$ where R is a correlational matrix whose entries differ from the entries in the obtained correlational matrix R_o by a mean order of magnitude comparable with their mean chance error (Thurstone, 2). In order that the vectorial configuration of F shall represent R uniquely it is necessary that the following inequality is satisfied, namely,

$$r \leqq \frac{(2n+1) - \sqrt{8n+1}}{2}$$

where n is the number of tests and r is the number of independent common factors or abilities. This theorem is presented here without the proof which will be published in the volume on factor theory.

The entries in the factorial matrix F of rank r may be thought of as the coordinates of the termini of n vectors in space of r dimensions. The scalar of the test vector j is h_j where h_j^2 is the communality of test j. If the coordinates of test j are augmented by the multiplying factor $1/h_j$ then the augmented coordinates represent the direction cosines of unit test vectors in the common factor space of r dimensions.

The unitary abilities are determined by rotating the coordinate

*Accepted for publication by Carl Murchison of the Editorial Board.

system of F so that each test vector lies in one or more of the coordinate planes or as near these planes as possible. This condition is obtained when the coordinate system of F is rotated so as to maximize the number of zero entries in F. Even if the coordinate system of F is rotated the factorial matrix reproduces exactly the correlation matrix. It is my belief that the coordinate axes so determined define the unitary abilities and the experience factors that are involved in the common factors of the n tests.

In case the rank of F is 3, the termini of the augmented unit test vectors can be plotted on the surface of a sphere. It is then found that a right spherical triangle can be drawn so that the tests are all represented either on the sides of this triangle or inside the triangle. The vertices of this right spherical triangle define unitary abilities and experience factors. The rotation can be effected by inspection of the sphere. If more than three dimensions are involved, the rotation must be effected analytically.

One result of this orthogonal transformation is that negative coordinates disappear. Hence it is not necessary to consider the problem of how to interpret negative abilities.

A set of fifteen tests of Brigham's (1, pp. 248-277) has been chosen as an example. The three factors of Table I were obtained by an improved centroid method. The table contains negative entries. The same table also shows the augmented coordinates. They are the direction cosines of the augmented test vectors with reference to the arbitrary coordinate axes of the centroid method. After rotation, the entries become positive or nearly zero as shown in columns G, V, and S. The three abilities can then be named by inspection of the tests. The names here given are V for verbality, S for space, and G for abstraction. The last three columns show what part of the variance of each test can be attributed to each of the three abilities. Column u^2 shows the uniqueness which is the part of the variance of each test which is not accounted for by the three common abilities. The coordinates of the three orthogonal points G, V, and S which are selected by inspection are as follows:

		I	II	III
G	abstraction	+.883	—.290	—.370
V	verbality	+.398	+.880	+.261
S	space	+.249	—.378	+.892

TABLE 1

No.	Name of test	Centroid coordinates I	II	III	Commu-nality h^2	Scalor of test vector h	Augmented coordinates I	II	III	Coordinates after rotation G	V	S	Variance due to each ability G	V	S	u^2
10	Synonym-antonym (nouns)	.644	.431	.147	.6221	.7887	.817	.546	.186	.389	.674	.129	.15	.45	.02	.38
2	Synonym-antonym (adjectives)	.577	.499	.069	.5867	.7660	.753	.651	.090	.339	.687	.017	.12	.47	.00	.41
5	Synonym-antonym (nouns)	.559	.448	.016	.5134	.7165	.780	.625	.022	.358	.621	—.016	.13	.39	.00	.48
3	Analogies	.716	.219	—.081	.5672	.7531	.951	.291	—.108	.599	.457	.023	.36	.21	.00	.43
4	Artificial language	.628	.102	—.018	.4051	.6365	.987	.160	—.028	.532	.335	.102	.28	.11	.01	.60
1	Definitions	.682	.141	—.136	.5035	.7096	.961	.199	—.192	.612	.360	—.005	.38	.13	.00	.49
8	Geometrical completion	.523	—.142	—.199	.3333	.5773	.906	—.246	—.345	.577	.031	.006	.33	.00	.00	.67
7	Arithmetical problems	.556	—.145	—.225	.3808	.6171	.901	—.235	—.365	.616	.035	—.007	.38	.00	.00	.62
9	Arithmetical proportions	.534	—.207	—.128	.3444	.5869	.910	—.353	—.218	.579	—.003	.097	.34	.00	.01	.65
6	No. series completion	.593	—.306	—.277	.5220	.7225	.821	—.424	—.383	.715	—.106	.016	.51	.01	.00	.48
15	Cord turning	.465	—.106	.112	.2400	.4899	.949	—.216	.229	.400	.121	.256	.16	.02	.07	.75
14	Block construction	.419	—.231	.139	.2482	.4982	.841	—.464	.279	.386	.000	.316	.15	.00	.10	.75
17	Dice counting	.623	—.312	.222	.5348	.7313	.852	—.427	.304	.558	.031	.471	.31	.00	.22	.47
11	Painted cubes	.605	—.338	.310	.5764	.7592	.797	—.445	.408	.518	.024	.555	.27	.00	.31	.42
18	Form learning	.413	—.056	.045	.1757	.4192	.985	—.134	.107	.364	.127	.164	.13	.02	.03	.82
		8.537	—.003	—.004												

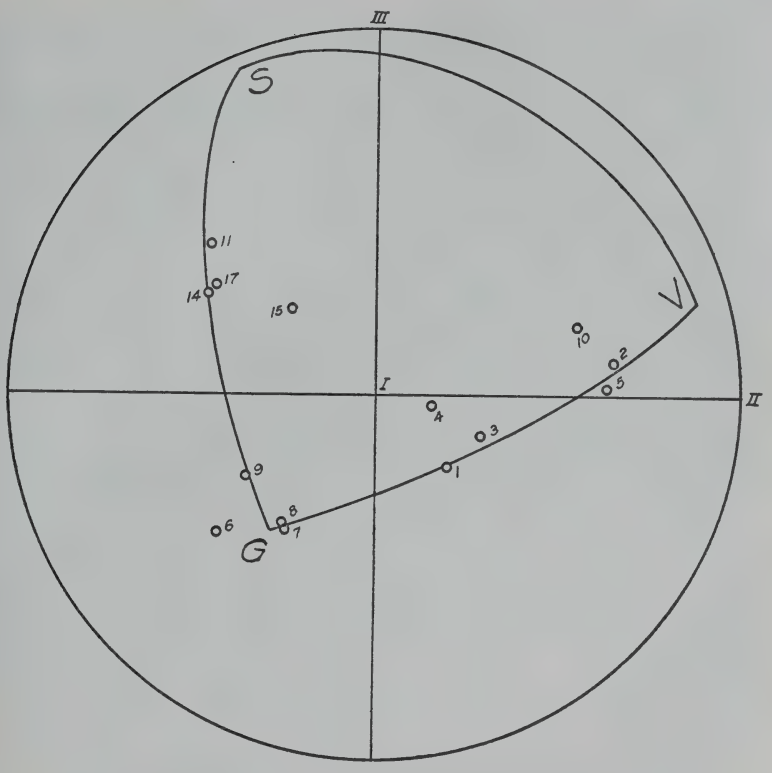

FIGURE 1

Figure 1 represents a sphere on which the termini of the augmented test vectors are plotted. The horizontal line represents the arbitrary factor II and the vertical line represents the arbitrary factor III. The first arbitrary factor is represented by an axis through the origin and normal to the plane of the drawing.

The right spherical triangle determines the three orthogonal points *G, V,* and *S.* Note that there are no tests in the *VS* plane. This makes the vertex *G* unique and it probably represents the general intellective factor of Spearman. In all the test batteries that we have investigated so far, one of the vertices is unique in that all of the test vectors have some positive projection on its vector whereas the other vertices determine reference vectors on which some of the tests have zero projections.

I have discarded the method of principal axes (3, p. 13, table 5) which I described two years ago. My reasons are as follows. The principal axes of a set of test vectors are determined by the grouping of the tests in the battery. If one of these tests is moved from one battery to another its factorial description will vary because of the fact that the principal axes are not the same for the several batteries. *If a test involves a unitary ability, the description of the test with reference to this ability must remain invariant when the test is moved from one test battery to another test battery which contains this unitary ability as a common factor.* This is a fundamental criterion for any method of isolating unitary abilities. Simple logical consideration shows that the method of principal axes can not meet this criterion. The method of rotation of the test vectors into the coordinate planes can satisfy this criterion. With the new method of rotation that is here proposed we may expect to find that a test remains invariant as regards its factorial description when it is moved from one test battery to another.

The fact that an orthogonal transformation exists by which most of the tests have one or more zero coordinates is not a chance matter. It probably represents an identification of the abilities at the vertices with phenomena extraneous to the test scores and their intercorrelations. I propose the hypothesis that *the reference abilities by which the negative factorial coordinates disappear and by which at the same time a large number of zero coordinates appear in the factorial matrix and by which the factorial coordinates become psychologically meaningful constitute unitary abilities or experiential factors.*

By unitary ability is here meant an ability which is significant because of its identification with phenomena extraneous to the test scores and their intercorrelations. It is my present belief that the unitary abilities have genetic significance and that it will be possible eventually to identify the special abilities genetically.

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LES HABILETÉS UNITAIRES

(Résumé)

Cet article a le but de décrire une méthode d'isoler les habiletés unitaires. Une des difficultés de l'analyse des facteurs a été celle de trouver une interprétation pour les habiletés négatives. On peut montrer qu'avec les données des tests psychologiques le système coordonné de la matrice des facteurs peut être mis en rotation de sorte que les habiletés négatives disparaissent et que les entrées nulles paraissent dans la matrice. Les nouveaux axes coordonnés ainsi déterminés définissent les habiletés de référence qui sont psychologiquement signifiantes. S'il s'agit d'une habileté unitaire dans un test, la description du test à l'égard de cette habileté doit rester non variante quand l'on enlève le test d'une batterie de tests et le met dans une autre batterie qui contient cette habileté unitaire comme facteur commun. Ceci est un critère fondamental pour n'importe quelle méthode d'isoler les habiletés unitaires. Une simple considération logique montre que la méthode des axes principaux ne peut pas satisfaire ce critère. La méthode de rotation des vecteurs des tests dans les plans coordonnés peut satisfaire ce critère. L'hypothèse ici proposée est que les habiletés de référence par lesquelles les coordonnés négatifs disparaissent et par lesquelles en même temps grand nombre de coordonnés nuls apparaissent dans la matrice des facteurs et par lesquelles les coordonnés des facteurs deviennent psychologiquement signifiants constituent des habiletés unitaires ou des facteurs empiriques.

Par habileté unitaire on veut dire ici une habileté qui est signifiante à cause de son identification avec des phénomènes en dehors des résultats des tests et de leurs intercorrélations. Je crois à présent que les habiletés unitaires ont une signifiance génétique et qu'il sera possible éventuellement d'identifier génétiquement les habiletés spéciales.

THURSTONE

EINHEITLICHE FAEHIGKEITEN

(Referat)

Der Zweck dieses Aufsatzes ist die Beschreibung einer Methode zur Absonderung einheitlicher Fähigkeiten. Eine der Schwierigkeiten der Faktoranalyse ist die Auffindung einer Deutung der negativen Fähigkeiten gewesen. Es kann gezeigt werden, dass bei psychologischen Testdaten das Koordinatensystem der Faktormatrize so gedreht werden kann, dass negativ Fähigkeiten verschwinden, und dass Nulleintragungen in der Matrize erscheinen. Die neuen Koordinatenachsen, welche so erreicht worden sind, bestimmen Beziehungsfähigkeiten, die psychologisch sinnvoll sind. Wenn ein Test eine einheitliche Fähigkeit enthält, muss die Beschreibung des Tests mit bezug auf diese Fähigkeit unveränderlich bleiben, wenn der Test aus einer Testbatterie in eine andere Testbatterie geschoben wird, welche diese einheitliche Fähigkeit als einen gemiensamen Faktor enthält. Dies ist ein Grundkennzeichen irgendeiner Methode zur Absonderung einheitlicher Fähigkeiten. Eine einfache logische Betrachtung zeigt, dass die Methode der Hauptachsen dieses Kennzeichen nicht trifft. Die Methode der Drehung der Testvektoren in die Koordinatenebenen kann dieses Kriterium befriedigen. Die hier aufgestellte Hypothese besagt, dass die Beziehungsfähigkeiten, durch die negativ Faktorkoordinaten verschwinden und durch die eine grosse Anzahl Nullkoordinaten in der Faktormatrize

zugleich ersheinen und durch die Faktorkoordinaten psychologisch sinnvoll werden, einheitliche Fähigkeiten oder experimentelle Faktoren ausmachen.

Einheitliche Fähigkeit heisst hier eine Fähigkeit, die wegen ihrer Identizifierung mit den Testergebnissen äusserlichen Phänomenen und derer Wechselbeziehungen bedeutsam ist. Ich bin der Meinung, dass die einheitlichen Fähigkeiten eine genetische Bedeutung besitzen und dass es möglich sein wird, die Sonderfähigkeiten eventuell genetisch zu bestimmen.

THURSTONE

THE EFFECT OF GENERAL ANAESTHESIA UPON THE EXPERIMENTAL REDUCTION OF VESTIBULAR NYSTAGMUS*

From the Department of Psychology of Northwestern University

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I. PROBLEM

Previous investigations[2] with both human and animal subjects have clearly demonstrated that the vestibular nystagmus which ordinarily follows a period of continuous bodily rotation may be significantly reduced—in some cases virtually abolished—by means of repeated elicitation under properly chosen experimental conditions. However, in spite of the consistency and decisiveness of the empirical evidence now available, there exists a sharp cleavage of opinion regarding the probable explanation of this phenomenon. In 1906, Abels (1) advanced the theory that the reduction of post-rotational nystagmus which occurs with repeated elicitation is due to some sort of functional change in the central nervous system, which he referred to as "habituation" (*Gewöhnung*); and many subsequent writers (5, 7, 8, 9, 20) have likewise been inclined toward the same interpretation. On the basis of considerations advanced by Bárány (2) in 1907, another group of writers (10, 11, 15, 18, 24) have been led to assume, on the other hand, that post-rotational nystagmus is "normally invariable," i.e., that any marked deviation in its duration from certain accepted norms is a definite sign of lesion or pathological change in the vestibular receptors of the inner ear or in the neural tracts communicating with these organs; the obvious implication of this point of view being that the reduction of nystagmus through repeated bodily rotation is dependent upon some type of injury to or structural derangement of the vestibular receptors or

*Accepted for publication by Carl Murchison of the Editorial Board and received in the Editorial Office, July 20, 1933.

[1]The junior writer is a National Research Fellow in the Biological Sciences.

[2]These studies have recently been reviewed elsewhere (20) and will therefore not be individually cited here.

afferent pathways. It has also been suggested (5, 23, 24) that the shortening of nystagmus with repetition might merely represent an "increased control of voluntary eye movements," i.e., a greater facility in "voluntary fixation of the gaze"; but since it has been shown (16, 20) that nystagmus is reduced by repeated bodily rotation with vision entirely eliminated, this hypothesis has become quite untenable. "Fatigue" and "sensory adaptation" have likewise been considered as possible explanations for the nystagmus reduction occurring with repeated elicitation but have been discarded because of the fact that this type of response modification, instead of lasting for only a few minutes or hours (as do the effects commonly classified under the two above categories), usually persists for an indefinite period of time (22).

The only two theories of nystagmus reduction which thus appear still to have a significant number of protagonists are: (*a*) that repeated bodily rotation, being an essentially unnatural (biologically unusual) type of stimulation, produces a purely "functional" change in the central nervous system, thereby rendering the organism *less responsive* to subsequent rotational stimulation, which, however, presumably continues to produce a normal effect upon the vestibular receptors as such; and (*b*) that repeated bodily rotation, as a result of the operation of purely mechanical factors, injures or in some way structurally modifies the vestibular receptors so as to make them *less sensitive,* thus reducing the intensity of the vestibular excitations passing to the central nervous system and consequently diminishing the vigor of the nystagmic responses.

II. Previous Experimentation

Apparently the first experimental attempt to determine which of the two foregoing hypotheses is correct was made in 1922 by Maxwell, Burke, and Reston (19). These writers found that rabbits whose nystagmus had been reduced by repeated bodily rotation gave a normally vigorous response when later stimulated by aural irrigation. To quote:

> Our experiments show that daily rotation of rabbits reduces, in some cases very markedly reduces, the after-nystagmus. We have attempted by the caloric test to discover the functional state of the labyrinth at the close of the series. We have no reason to believe that the animals have been injured by the amount and rate of rotation [one revolution in two seconds]

> to which they have subjected. It is true that Wittmaach (1909) has reported and de Kleijn and Magnus (1920) confirmed the statement that rotation (centrifugalization) at a high rate of speed [33 revolutions per second] can produce destructive changes in the labyrinth. But the latter observers have used even this high speed to differentiate between the canals and the otoliths, since only the otoliths are injured by this means. Now in our experiments we are dealing with reactions to rotation in the plane of the horizontal canals, in which we believe the ampullae of the horizontal canals are mainly and the otoliths possibly not at all concerned. . . . We believe the results warrant the assumption that no injury has been done but that through habituation the organism has been rendered less responsive.

In 1925 Dunlap (7) reversed the procedure used by Maxwell, Burke, and Reston and investigated the effect of repeated caloric stimulation upon the sensitivity of rabbits to bodily rotation. He also arranged his experiment so that it would indicate whether the effect of repeated caloric stimulation is specific for the ear stimulated or whether the response produced by stimulation of the other ear is also affected. It was found that daily irrigation of one ear (the left) progressively shortened the nystagmus elicited by this type of stimulation and at the same time produced a marked reduction in the duration of the nystagmus elicited by subsequent caloric stimulation of the previously unirrigated (right) ear. In this connection Dunlap says:

> From the marked effect on the right ear nystagmus by left ear irrigation, it is fairly obvious that the adaptation produced by the irrigation was not entirely "peripheral"; that is, it was not an effect on the receptors or other mechanisms within the semicircular canals or vestibules of the ear on the side irrigated; but was in part "central," that is, it was an effect on the nervous mechanism connected with both ears.

Later in the experiment, after both ears had been subjected to repeated caloric stimulation, the rabbits were tested for post-rotational nystagmus. The results were such as to lead Dunlap to the concluson that "obviously, the caloric stimulation had produced a certain adaptation to rotary stimulation also."

A year later Gould (12) made a detailed histological study of the labyrinths of rats in which nystagmus had been reduced by means

of repeated rotation. In regard to his findings, he says, ". . . it early became apparent that a possible structural basis for the abnormal reactions reported must be sought elsewhere than in injury to or abnormal development of the parts of the vestibular ear."

Although bearing less directly upon the problem than the foregoing studies, the findings reported by Holsopple (14), in 1924, lend further support to the supposition that the reduction of nystagmus through repeated bodily rotation is dependent upon some sort of process occurring in the central nervous system. This writer found with human subjects—and Mowrer (20) has recently confirmed with pigeons—that, in order for repeated bodily rotation to be most effective from the point of view of nystagmus reduction, it is essential for the alternate periods of rotation and rest to last for several seconds, preferably long enough for the nystagmus produced by one type of stimulation (acceleration or retardation) to die away before the occurrence of the opposite type of stimulation (retardation or acceleration).[3] In other words, if the successive stimulations produced by the starting and the stopping of the subject occur in too rapid succession, little or no nystagmus reduction will result from repeated rotation. On the basis of this finding, Holsopple concluded that nystagmus "is *not* reduced merely because of the frequent occurrence of its stimulus" but rather "by virtue of having frequently run to normal completion." This latter statement can scarcely be said to constitute an explanation of *why* the nystagmus reduction occurs, but it does afford a useful description of one of the conditions essential for the production of this phenomenon and emphasizes, moreover, the improbability of this type of response modification being dependent upon effects restricted to the vestibular receptors.

Although the experimental evidence reviewed in the foregoing paragraphs affords substantial support to the "central" theory of nystagmus reduction, other considerations seem to favor the "peripheral" hypothesis. A systematic study of the permanence of the nystagmus reduction produced by repeated bodily rotation has not yet been reported, but such incidental observations as have been

[3]It is, of course, a well-established fact that it is not bodily rotation as such which produces vestibular nystagmus but rather the change in angular velocity, i.e., the acceleration and retardation, incident to the starting and stopping of the subject.

made (22) indicate, as intimated above, that there is relatively little recovery, even after some weeks or even months, and strongly suggest that the reduction effect, if not strictly permanent, is at least fairly enduring. This fact does not, of course, necessarily prove that nystagmus reduction through repeated bodily rotation is due to definite and irreparable damage inflicted upon the vestibular apparatus; but it is certainly quite consistent with such an interpretation.

The apparent plausibility of this hypothesis is further increased by the fact that individuals who have served as subjects in repeated rotation experiments have occasionally complained of permanently impaired equilibrium (particularly in the dark) and of certain sensory disorders (notably hearing difficulties), the development of which they have been inclined to attribute to their previous rotational experiences. Moreover, Griffith (13) has reported a study in which a group of white rats which were continuously rotated for a period of several weeks developed serious disturbances of equilibrium and locomotion. Detlefson (4) and Dorcus (6) have shown that the development of the motor peculiarities manifested by Griffith's animals was probably wholly unrelated to the continuous rotation *per se;* and Mowrer (21) has recently advanced considerations which suggest that the equilibratory and other disturbances reported by the human subjects were probably imaginary or, if real, due to causes having no connection with the repeated rotation. Nevertheless, the possibility of genuinely deleterious consequences resulting from continuous or repeated bodily rotation has such serious implications that the foregoing reports of injurious effects cannot safely be excluded from consideration.

In an experiment reported in 1926, King (16) found that the cerebral hemispheres are not necessary for nystagmus reduction in the pigeon, and concluded on the basis of this finding that this effect "does not seem to be of the nature of a learning process." This inference is obviously predicated by the implicit assumption that "learning" can occur only in the cerebrum, which assumption Beritoff (3), also using pigeons as subjects, has explicitly contradicted. However, King's further conclusion that the reduction of nystagmus by means of repeated rotation "must be effected in the lower centers of the central nervous system or in the receptors" would appear entirely justified, although it leaves still unanswered the important

question as to which of these two alternate anatomical references is the true locus of the modification process and throws no light upon the specific nature of the organic changes involved.

III. Procedure and Results

In view of the practical as well as theoretical significance of this problem and of the not entirely unequivocal nature of the experimental evidence thus far published, it seemed to the present writers worth while to attempt to determine whether nystagmus reduction would occur in subjects which were deeply anaesthetized just prior to each session of repeated bodily rotation;[4] the assumption being that if the reduction were dependent upon changes produced in the vestibular receptors by the purely mechanical effects of the rotation, the repeated rotation should be quite as effective with anaesthetized as with normal subjects, but if, on the other hand, the reduction were dependent upon a "functional" change in the central nervous system, then the suppression of neural activity in anaesthetized subjects should prevent them from showing any modification of response when subsequently tested under normal physiological conditions.

Forty adult common pigeons were accordingly selected as subjects and were divided into four equal groups. The ten birds constituting the first group were given a standardized test for post-rotational nystagmus; and then, after a 16-day interval during which they were left unmolested in their loft, they were re-tested for nystagmus in exactly the same manner in which they had been originally tested. The natural expectation that there would be little or no difference in the results obtained on the two tests is borne out by the figures presented in Table 1.

A second group of ten birds was subjected to exactly the same procedure as just described except that, on the 1st, 4th, 7th, 10th, and 13th days of the 16-day interval between the original and final tests, the birds in this group were deeply anaesthetized by appropriate injections of a newly developed compound which is obtainable under the trade-name of "Nembutal."[5] As shown by the results

[4]This experiment was suggested by Professor Knight Dunlap to the junior author while the latter was a graduate student at the Johns Hopkins University.

[5]This compound—the complete chemical designation of which is Sodium Ethyl [1-methyl Butyl] Barbiturate—is prepared by the Abbott Laboratories, North Chicago, Ill., and is related to the common hypnotic known

TABLE 1

		Original test		Final test	
		Average nystagmus	Probable error	Average nystagmus	Probable error
Group I Not rotated and not anaesthetized	Clockwise rotation	21.2″	(±0.93)	19.7″	(±0.80)
	Counterclockwise rotation	21.3″	(±0.99)	18.2″	(±0.88)
Group II Anaesthetized but not rotated	Clockwise rotation	23.1″	(±0.73)	22.5″	(±0.84)
	Counterclockwise rotation	23.0″	(±0.76)	23.1″	(±0.91)
Group III Rotated but not anaesthetized	Clockwise rotation	20.1″	(±0.71)	8.1″	(±0.35)
	Counterclockwise rotation	19.7″	(±0.76)	8.4″	(±0.36)
Group IV Rotated and anaesthetized	Clockwise rotation	19.9″	(±0.64)	19.6″	(±0.64)
	Counterclockwise rotation	20.5″	(±0.69)	19.6″	(±0.59)

presented in the table, the repeated administration of this anaesthetic produced no change in the duration of the post-rotational nystagmus between the original and final tests; and there was no evidence of any other type of physiological disturbance.

A third group of ten birds was tested and re-tested for nystagmus in the manner indicated above. However, on the 1st, 4th, 7th, 10th, and 13th days of the interval between nystagmus tests, these birds were subjected to an intensive program of repeated bodily rotation, consisting of 20 periods of rotation at the rate of 20 turns in 30 seconds, with interpolated rest periods also of 30 seconds' duration. The results obtained on the final test show that the repeated rotation brought about an average reduction of about 60 per cent in the duration of the nystagmus of this group.

A fourth and final group of ten birds was subjected to the same procedure as the third group, except that the birds in this fourth group were given the repeated rotation while under deep anaesthesia, which was induced by the same method as used with the second group of birds. In order to keep their heads in a normal position and to assure the same type of stimulation in these anaesthetized birds as in unanaesthetized subjects, it was necessary to use a special type of head holder during the repeated bodily rotation. Although the holder was designed in such a way as to leave complete freedom for the occurrence of nystagmus, this response was seldom manifested, thus indicating the effective suppression of the neural mechanisms ordinarily involved. Following the five sessions of repeated bodily rotation under the conditions just described, the birds in this fourth group were re-tested 16 days after the original test under normal physiological conditions. That the anaesthetization of these subjects just before being subjected to the repeated rotation had rendered them entirely immune to the reduction of nystagmus occurring under similar circumstances in unanaesthetized subjects is clearly revealed by the data presented in Table 1 and is definitive evidence of the neural character of the process involved in this type of response modification.

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as "Barbital." The writers have found that for use with pigeons an intraperitoneal injection of 0.18 cc. per 100 grams of body weight of a solution of 1 gr. of Nembutal dissolved in 4 cc. of distilled water produces a deep surgical anaesthesia lasting from one to two hours, which is not followed by the nausea and other distressing sequalae characteristic of ether and chloroform.

IV. Discussion

Since the occurrence of head nystagmus necessarily alters the pattern of vestibular stimulation produced by bodily rotation and since this response was almost wholly absent in the anaesthetized subjects, it may be objected that these birds did *not* receive the "same type of stimulation" during the repeated bodily rotation as did the unanaesthetized subjects and that the absence of an habituation effect in the anaesthetized subjects was therefore perhaps due to this difference. In an earlier study (20) it was found that repeated bodily rotation of pigeons in which the occurrence of nystagmus is forcibly prevented nevertheless results in a conspicuous reduction in the nystagmus, as subsequently tested with the head entirely unrestrained. It was also discovered at the same time that repeated bodily rotation of pigeons with vision unobstructed produces absolutely no reduction in their vestibular nystagmus, as later tested with the subjects hooded, even though during rotation with vision birds regularly show a far more vigorous nystagmus than they do when they are rotated without vision (ie., hooded). In short, it was found that nystagmus reduction may result from repeated bodily rotation even though the nystagmus is forcibly inhibited during this rotation and that mere repeated occurrence of nystagmus as such is, on the other hand, no guarantee that the repetition will lead to a reduction. Although the type of vestibular stimulation received by the birds in Group III was admittedly not strictly identical from all points of view with that received by the birds in Group IV, it was certainly the same as regards the plane of stimulation (i.e., the horizontal canals were in both cases exclusively, or at least predominantly, affected); and there is no reason to suppose that the difference in the pattern of stimulation which unavoidably was present was at all significant from the point of view of the difference in the type of results obtained.

In the light of the results of the present investigation and of certain earlier experimental studies cited above, it is evident that the reduction of post-rotational nystagmus, which occurs with repeated elicitation under suitable circumstances, is dependent upon some sort of purely "functional" change in the central nervous system and is in no way due to injury to or structural dearrangement of the vestibular receptors. Previous writers (1, 6, 8, 20) have

conjectured that this neural change might be of the nature of a learning process or of a progressive functional breakdown of normal integrated functioning. In a recent publication one of the present writers (21) has suggested a somewhat more specific hypothesis.

It was formerly believed that the persistence of vestibular nystagmus after the withdrawal of its actual, objective stimulus (e.g., after the end of bodily rotation) was due to a continued disturbance (flowing of endolymph, distortion of the cristae acousticae, or the like) within the vestibular receptor apparatus. The more modern point of view is that the persistence of nystagmus after the removal of its objective stimulus is due to the action of a so-called "after-discharge" mechanism located in the primary vestibular nuclei, which, once properly activated, is self-exciting and causes effective innervation to flow to the muscles involved in nystagmus for some time after the cessation of the original, external stimulation. Lorente de Nó (17) has recently reported a careful study of this mechanism and has shown, partly on theoretical, partly on empirical grounds, that any change in the excitation threshold or in the duration of the refractory phase of the neurons constituting this after-discharge mechanism might be expected to produce a change in the length of time it will continue to discharge after the presentation of a stimulus of given intensity. For reasons which cannot pertinently be given here (cf. Mowrer, 21), it is suggested that repeated presentation of the type of stimulation produced by more or less prolonged bodily rotation may modify either the excitation threshold or the refractory phase of the neurons composing this after-discharge mechanism in such a way as to result in the progressive reduction of nystagmus. In view of the somewhat speculative character of the assumptions upon which it is based, this hypothesis is naturally advanced as highly tentative and as subject to such qualification or revision as may subsequently seem necessary.

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L'EFFET DE L'ANESTHÉSIE GÉNÉRALE SUR LA RÉDUCTION EXPÉRIMENTALE DU NYSTAGMUS VESTIBULAIRE

(Résumé)

Les investigateurs antérieurs ont décisivement démontré que le nystagmus vestibulaire produit par la rotation continue du corps peut être réduit d'une façon significative en durée et en magnitude au moyen d'une mise en action répétée dans des circonstances appropriées. On a fait l'étude actuelle dans le but de déterminer si ce type de modification des réponses est dépendant d'une lésion spécifique des récepteurs vestibulaires de l'oreille intérieure ou de quelque procédé dans le système nerveux central. On décrit des résultats qui montrent que la rotation répétée du corps ne produit pas de réduction du nystagmus si les sujets (des pigeons) sont soumis à la rotation répétée pendant qu'ils sont dans une forte anesthésie chirurgicale. Sur la base de ce résultat et d'autres résultats expérimentaux cités, on conclut que la réduction du nystagmus vestibulaire au moyen de la rotation répétée du corps (à des vitesses modérées) n'est dépendante ni d'une lésion ni d'aucun type de dérangement de la structure des récepteurs vestibulaires mais plutôt d'un changement seulement "fonctionnel" dans le système nerveux central. On offre une suggestion tentative à l'égard de la nature probable de ce changement névrale.

FEARING ET MOWRER

DIE WIRKUNG EINER ALLGEMEINEN ANAESTHESIE AUF DIE EXPERIMENTELLE VERMINDERUNG DES VORHOFSNYSTAGMUS

(Referat)

Es ist von früheren Forschern nachgewiesen worden, dass der Vorhofsnystagmus, der durch beständiges körporliches Drehen erzeugt wird, bedeutend vermindert werden kann, sowohl an Dauer als auch an Grösse vermittelst wiederholter Hervorrufung unter geeigneten Umständen. Der Zweck dieser Untersuchung zielt darauf hin, zu bestimmen, ob diese Art Reaktionsveränderung von spezifischer Verletzung der Vorhofsempfänger des inneren Ohres, oder von irgendeinem Vorgang des zentralen Nervensystems abhängig ist. Die Ergebnisse zeigen, dass wiederholtes körperliches Drehen keine Verminderung des Nystagmus erzeugt, wenn die Versuchstiere (Tauben) einer tiefen chirurgischen Anästhesie ausgesetzt werden. Auf Grund dieses und anderer Befunde, die angegeben sind, wurd gefolgert, dass die Verminderung des Vorhofsnystagmus durch wiederholtes körperliches Drehen (zu mässigen Geschwindigkeiten) von einer Verletzung oder irgendeiner Art Strukturstörung der Vorhofsempfänger nicht abhängig ist, sondern vielmehr von einer rein Funktionsveränderung des zentralen Nervensystems. Ein vorläufiger Vorschlag in bezug auf die wahrscheinliche Natur dieser Nervenveränderung wird gemacht.

FEARING UND MOWRER

CHANGING CONCEPTIONS IN PHYSIOLOGICAL PSYCHOLOGY*[1]

(With Special Reference to the Contributions of Paul Weiss)

From Massachusetts State College

Harry R. DeSilva and Willis D. Ellis

The ideas of nerve physiology and psychology seem to be undergoing a major shift of emphasis, away from the reflex arc, point-to-point theory of neural function toward a theory of dynamic plastic specificity of neural function.

The older nerve physiology, based upon earlier studies of action currents, endeavored to explain the specificity of communication between the central nervous system and periphery chiefly in terms of the following conditions: (1) point-to-point connection between specific end-organs and specific centers in the central nervous system; (2) number of fibers involved; (3) frequency and spacing of the propagated action current impulses. The well-established all-or-none law for a long time seemed to rule out the possibility of amplitude of nerve impulse being used as an additional variable condition. With these few conditions, which had ample experimental support, early workers developed a picture of coordinated activity based closely upon structural differentiation and inherited reflexes.

Present notions of the most advanced research workers about the nervous system have gone far beyond this older mechanistic reflex point of view. Thus Adrian, who has done probably more than any one else to improve our knowledge of action currents, concludes his *Mechanism of Nervous Action* with this significant remark: "They [the nerve fibers] have a fairly simple mechanism when we treat them as individuals; their behavior in the mass may be quite another story, but this is for future work to decide." Similarly Sherrington and especially Lashley have done much to break down the oversimplified views of early reflexology. Thus Lashley in his *Brain Mechan-*

*Accepted for publication by Carl Murchison of the Editorial Board.

[1]We are indebted to Dr. Hudson Hoagland for a critical reading of our manuscript and for suggestions regarding the introduction.

isms and Intelligence states: "The distinguishing feature of the reactions which we designate as 'behavior' in contradistinction to reflex is that they are not conditioned by the excitation of specific limited receptors or by stereotyped central mechanisms but involve features of plasticity which have never been expressed in terms of the reflex theory and which imply, as we have seen, an independence of specifically differentiated conduction paths. . . . [p. 164]. These phenomena all point to a functional organization independent of differentiated structure to some more general energy relations within the central nervous system" (p. 167).

An important contributor to the newer point of view is Paul Weiss, whose experimentally substantiated "resonance theory" is probably as significant for the motor side of the nervous system as Lashley's equipotential theory is for the cortex.

Dr. Paul Weiss has recently moved from Germany to the United States (University of Chicago). However, since his publications with one or two exceptions have appeared in German and Austrian periodicals, he is relatively unknown outside of continental Europe. We propose, therefore, to summarize Weiss's original contributions to the new dynamic conception of the nervous system.

Weiss's Experimental Findings

Over a period of thirteen years Weiss has transplanted whole limbs as well as separate muscles in amphibia (chiefly salamanders and toads) in a concerted attack on the problem of how muscles are innervated. By disarranging Nature's scheme of things in the peripheral nervous system of these organisms he has found answers to problems that puzzled earlier investigators and has discovered some new facts which seem to necessitate a new theory of muscle innervation.

Transplantation of Limbs. In 1922, working with the larvae of *Salamandra maculosa,* Weiss was successful in transplanting functionally developed limbs from one organism to another in 70 or more cases.[2] An incision having been made near the "host" limb, a supernumerary leg or arm was brought from another organism and inserted. Severance of part of the nerves leading to the host limb furnished the nerve supply. Observation of subsequent behavior by

[2]In his later work, done almost entirely with toads, Weiss has used over 400 specimens.

the supernumerary limb was in each instance continued for at least one year (15, pp. 636 ff.). Since the extra limb was usually completely a part of the host organism after from two to three months, ample opportunity was thus given to observe its behavior and that of the host in great detail. In all cases where the supernumerary limb received a part of its reinnervation from the limb level of the spinal cord it finally came to perform precisely as did the nearby host limb (16, p. 241). Weiss refers to this as the *homologous* function of the supernumerary and host. Thus it was found that every movement of the host limb was accompanied synchronously by the same movement in the supernumerary. Ankle (wrist) or knee (elbow) flexions in one were accompanied by simultaneous flexions in the corresponding region of the other. When an arm was inserted in the region of the host's leg, it performed homologously with the host limb, e.g., by bending at the elbow when the host bent its knee, bending the wrist with the host's ankle flexions, etc. (15, pp. 641 f.).

Right and left were also found to be immaterial since supernumerary right or left limbs were transplanted to either left or right side of the host organism without impairment of subsequent homologous functioning (26, p. 359). Nor were these activities dependent upon the *location* of the supernumerary relative to the host limb, for every possible position in the vicinity of the host limb—cranial, caudal, ventral, and dorsal—was chosen for the implantation of supernumeraries, without at all affecting the homologous functioning of the two. Indeed, even orientation of the extra limb made no difference, for implantation either foot-forward or -backward or to either side did not modify homologous functioning.[3] In a word, no matter where or how the supernumerary was inserted, perfect synchronization between corresponding muscles in the supernumerary and the host limb followed. A study of proprioceptive reflexes evoked from the supernumeraries revealed the ability of the animal to identify which joint of the supernumerary had been moved by the experimenter and in which direction, for, when the experimenter flexed or extended the knee (elbow) or ankle (wrist) of either, the corresponding joint of the other responded accordingly.

[3]In one experiment the upper arm was removed and the elbow inserted where the shoulder had been. The hand nevertheless continued (after recuperation) to function as a hand, never as an elbow, whose place relative to the body it now occupied (19, p. 12).

Case of a Frog with Natural Supernumerary Limbs. Convincing proof that nothing about the experimental situation had been responsible for these phenomena was supplied by the fortunate discovery of a "freak" frog that possessed two fully developed supernumerary legs situated caudally to his left foreleg [photograph in (25, pp. 672 f.)]. All the phenomena of homologous functioning were exhibited by the three "left" legs of this specimen although one of the supernumeraries had the anatomical organization of a right leg. The movement of the supernumerary right foreleg was unique in that it "tried" to act as a left foreleg. Never did this supernumerary foreleg function homologously with the animal's own right foreleg (26, p. 359).[4]

Data on Nerve Regeneration. When a nerve is severed, all nerve tissue peripheral to that point degenerates. Conversely, however, the centrally connected, proximal nerve stump at once begins to shoot out a veritable spray of nerve fibers which, like the roots of a plant seeking water, branch and re-branch as they push out from the point of severance. Like roots, some find their way into suitable paths (e.g., the now degenerated nerve pathways) along which they may grow; others encounter obstacles which bring them to a halt.

In no case was there an increase in the number of motor fibers leaving the spinal cord after a transplanted limb had been fully supplied with new nerves. Also, since the incisions were made at many different places, quite different host nerves in the different specimens were therefore caused to innervate the supernumeraries and reinnervate the host (15, p. 659; 19, pp. 21 f.). That is to say, nerve A, formerly connected in the host with *a,* is now connected *in the supernumerary* with *a, b, c, ... n.*[5] In the host, A can, of course, re-establish direct contact with the original *a,* but since *only*

[4]This and other similar evidence [much of which has been supplied by Detwiler (2)] shows that excitations do not leave the spinal cord *en masse* along its length. The excitation patterns leaving the right and left sides of the cord are not identical. Likewise, there are lengthwise subdivisions along the cord corresponding to certain functional regions of each side of the body. There are certain areas along the animals' sides where supernumerary limbs remain dormant. Also it should be noticed from the text that an arm transplanted to the region of its host's leg functions with that leg, not with either of the animal's own arms.

[5]Kennedy (6) severed all four motor nerves leading into the foreleg of a dog and then caused one of these (*musculo-spiralis*) to innervate the muscles of the entire leg. No disturbance of coordinated behavior resulted.

A supplies the entire supernumerary limb with nerve tissue,[6] we know, therefore, that the supernumerary A leads to *b, c, d, ... n as well as to a.* This is the great value of a transplantation technique in the study of nervous connection, for, since we know (as shown by the phenomenon of homologous functioning) that *a, b, c, ... n* operate precisely the same in both host and supernumerary, we therefore know also that direct connections in the host between *A-a, B-b,* etc., are not necessary and hence that any theory postulating such connections as requisite for innervation must be in error (15, p. 640; 25, pp. 681 f.).

Transplantation of Muscles. In another study Weiss succeeded in transplanting the *gastrocnemius* muscle from the right leg of an animal to a position in the back of the same animal. Successful transplantation of a muscle was achieved by keeping the muscle under tension in its new surroundings. Having put the muscle under tension, Weiss next inserted the cut end of the left eighth nerve[7] into a slit made lengthwise in the muscle, sewed up the wound, and allowed two or three months to elapse for regeneration.

After regeneration and reinnervation the transplanted *gastrocnemius* muscle, the normal left leg *gastrocnemius* muscle, and a control muscle such as the *ilio-fibularis* muscle were severed at one end, connected to muscle levers, and kymographic tracings taken of their natural spontaneous and reflex muscular activities caused by mechanically stimulating the forelimb of the animal. The tracings of responses by the transplanted and the normal *gastrocnemius* muscles corresponded exactly in onset, duration, and intensity of activity. A transplanted *semitendinosus* was found to be similarly associated in function with the normal *semitendinosus,* a transplanted *tibialis* with the normal *tibialis,* and so on, irrespective of location and the entirely abnormal nerve connections.

The Excitation. Since the limited number of ganglion cells which were originally connected with a limited section of muscle

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[6]That only A is involved was demonstrated by later studying the ganglion cells innervating the region of the incision; no new cells were found (photograph, 19, p. 19). This and kindred histological evidence indicates that A alone is responsible for innervating the supernumerary muscles *a, b, c, ...n.*

[7]Weiss also reports work with other muscles and other nerves in the same study (28, p. 615). Since the general conclusions are the same regardless of the specific muscle or nerve used, we have limited our report to work done with the *gastrocnemius* muscle and the eighth nerve.

are, after the introduction of a supernumerary limb, then connected not only with this same section but with the entire musculature of the implanted limb, certain ganglion cells now function with more than one muscle. Thus, as we have seen, an excitation along fiber A which originally induced a response *only* in muscle *a* may now bring about responses in *a* and/or *b, c, ... n.* In other words Weiss finds that under his experimental conditions even if the central nervous system has at its disposal only one route to two or more end-organs it ennervates those end-organs separately and independently. The coordinated function of the periphery may come to correspond perfectly to the central determination of coordination even when the natural anatomical connections are disarranged differently in each animal (19, pp. 41 f.).

Weiss is skeptical of the predominant functional rôle usually assigned to action currents (19, p. 84). In the first place he points out that artificial (e.g., electrical) stimulation will *not* cause homologous functioning. If A reaches *a, b, c, d,* and A is electrically stimulated, then *a, b, c, d,* all respond rather than, say, only *b, c, d,* when A is naturally stimulated (16, p. 249). Since most investigations upon action currents have employed artificial stimulation, Weiss argues that the results obtained are not as significant for the task of understanding normal organic functioning as his own findings. Secondly, "messages" passing down one fiber can innervate one muscle and fail to innervate another muscle. This indicates that action currents and anatomical specificity are not sufficient in themselves to explain organized response. Weiss believes that the *muscles, end-plates,* and possibly the *motor fibers* (provided the latter acquire specificity under the dominance of the particular muscle) *must themselves possess properties of selectivity* which enable them to respond to the appropriate action currents and not to others which reach them.

That action currents probably play an important part in nerve conduction Weiss does not at all doubt, but to suppose that they *alone* are sufficient to explain coordinated muscular activity in an intact organism seems to him to place an exaggerated emphasis on their significance. His evidence indicates a dynamic coordinated interaction between (1) a central broadcasting system, (2) transmitting motor nerves, and (3) a motor receiving system, which we can understand only in terms of the properties of all three.

Theoretical Interpretations

Inadequate Explanations. We have already anticipated and rejected one type of explanation. It might be that A, leading as it normally does in the host to *a,* would naturally find its way to the corresponding *a*-region in the supernumerary as well as re-establishing its direct contact with *a* in the host. But even if this did occur (and in the supernumerary A certainly does reach *a*) some of the A-branches also reach *b, c, d, ... n,* in the supernumerary. If *A-a* is re-established in the host, that is immaterial since our index, the supernumerary, supplies us with information enabling us to admit this (host) possibility and still reject the theory of direct connections as necessary for proper innervation.[8]

A more difficult argument to answer is that the supernumerary "learns" to function homologously with the host limb. It is an experimental observation that for some months after the operation practically no activity can be detected in the supernumerary limb; also that at first its behavior is weak and what we might call tentative (19, p. 51). Is it not reasonable to believe that this limb, at first gradually, then ever more swiftly, *learns* to coordinate its functions with the host limb? Or, if this ascription of learning to the limb is too anthropomorphic, does not the central nervous system learn to maneuver the supernumerary homologously with the limb it has all along been guiding?

That the learning hypothesis is completely at error will be seen from the following considerations. When the supernumerary limb begins to behave at all it never carries out any act alone and whatever it does is in complete, if at first feeble, homology with the host. But what of its manner of functioning? Here is a point most energetically emphasized throughout all of Weiss's writings. The supernumerary limb behaves absolutely correctly and, eventually, with full force as the limb that it is, i.e., "limb-correctly" (*organrichtig*); never "body-correctly" (*körperrichtig*) except when the experimenter has inserted it in such a way as to make its operations benefit the host (19, p. 9). A leg dorsally inserted relative to the host leg will move forward, press down (as if upon a support), then swing back toward the tail in perfect homology with the ambulating host limb—but all the time merely through the air!

[8]See also the Kennedy findings mentioned above.

Indeed the fact that in this case the supernumerary encounters practically none of the gravitational resistance being overcome by the host leg permits the supernumerary to get just a little ahead of the host limb, but when the animal is suspended by a belt in the air and thrashes its legs about, or when it swims, then completely synchronous behavior may be observed.[9]

How does it happen that if "learning" were responsible for the supernumerary limb's behavior, just this particular, homologously coordinated behavior should be learned and *never* any other? But take the case of limbs implanted hindside foremost. The supernumerary limb now extends its foot caudally when the host is extending its foot cranially, and so on (15, p. 643). Thus it happens that for the entire life of an organism thus encumbered every act of the host limb is opposed and hindered by the supernumerary—and there is *never* any changing (15, p. 639). Obviously if anything at all in the supernumerary limb's behavior were due to learning, *some* adaptive modification of this situation would result, but this is never the case.[10] From these and other considerations proposed by Weiss but unnecessary to repeat here, the explanation by learning seems thoroughly ruled out.

More Probable Explanations. Specificity of response is the thing which must be accounted for. Direct connection can be completely upset and even reversed without affecting specificity of response, hence our search must turn elsewhere (25, p. 681). The theory of re-established direct connection fails, as does that of learning. Let us first pause for a moment to inquire just what we mean in general by specificity.

Three types of specificity may be distinguished: spatial, material, and temporal. A sieve separates larger particles from smaller ones and is to that extent an instance of *spatially* specific discrimination. But its action is too crude for the sort of thing in which we are interested because no distinction is made by the sieve among the smaller or larger particles themselves (19, p. 87). *Material* specificity comes much closer to satisfying the conditions required by our problem. One thinks, for example, of the effect of drugs upon

[9]Moving-picture records were obtained of this study (see illustrations in 20).

[10]The six-legged frog is a striking example of this inability to adapt on the part of the normal supernumerary limb (25, p. 674).

specific organs of the body; or of the manner in which hormones, carried by the blood stream to every part of the body, nevertheless affect only specific bodily functions (19, pp. 87-91). A still closer approximation, however, is that of *temporal* specificity such as we find in acoustics where the periodicity of one tuning fork *specifically* corresponds to that of another—and to nothing else (12, pp. 91-106).

Suppose now that we consider a still more refined type of specificity in which communication through the whole mass of nerve tissue of a certain bodily region (compare acoustic communication through air) is effected by periodic changes of state [*periodische Zustandsänderung*] throughout that mass (19, p. 92); and suppose further that the end-organs of this region were, like attuned forks, variously sensitive to these changes of state. This would supply us with a kind of specificity in perfect agreement with the experimental findings. A change of state in A might, seen from this point of view, be such as to activate (i.e., "excite") *a, b, c, ... n* or any one of these or any combination according to the state promulgated in A (19, p. 33). The specificity is thus seen as lying not in the fixity of certain pathways but in the different responsivenesses of the end-organs[11] and correlative difference of excitation propagated from the central nervous system (19, p. 64; 24, p. 1). Moreover, a whole pattern of activating conditions may suffuse A simultaneously and call into action exactly that same pattern of end-organ functions, for, while all components of the excitation pattern are conveyed to all end-organs of that region, only those muscles will respond for which a proper, specific component is contained in that pattern. Just as the air will conduct thousands of *different* vibration frequencies, so may the nerve tissue do this, and similarly, just as these vibration frequencies will excite only those receptors (resonators) properly attuned to these frequencies, so may the changes of state in the nervous system in a given region call certain muscles into activity and not others (15, p. 663; 19, p. 95).

This, then, is Weiss's "theory of excitation specificity and excitation resonance" (19, p. 63), a theory which, however, we are enjoined *not* to confuse with the mere analogy taken from acoustics (physical vibrations and physically separate, unrelated resonators). It is with misgivings and hesitation that Weiss permits himself to

[11]Weiss believes that possibly the whole motor unit, *including the peripheral motor branch,* might have to be considered to be the tuned "end-organ."

use the not completely satisfactory term "resonance" (19, p. 103), and we would do him an injustice here were we less explicit on this point. Weiss sees the specificity of nervous communication as a matter more for mathematical than for physical study; he is dealing with a system that changes its states in certain ways, not with a sender, vibrations, a receptor—although the comparison is useful in an exposition of the theory. Especially dangerous is the term "vibration," for Weiss is careful to point out that he does not mean physical trembling in the nervous medium (15, p. 663; 19, p. 92). Little is known of the nature of nervous "vibrations" just as little is known of ether vibrations, yet we do know something of the dynamic processes in both cases. And this is why, as regards the available information about the nervous system, it appears that the resonance theory of nervous action accounts for and interprets these facts more adequately than any other theory thus far proposed, even if the physical nature of "resonance" itself cannot as yet be understood. It is a theory based on the significant processes or operations, not upon the nature of the mechanism.

One final word in the explication of this theory. It might be objected that the resonance principle implies an incredible waste of energy since a certain excitation is conveyed to *all* the end-organs of a region even though only *one* of them is in a position to respond to it. This objection rests upon a misconception of Weiss's doctrine, for nervous communication as he understands it is *not* a form of energy but only a type of charge in the entire region of excitation (19, pp. 48 f.). But even though energy might be expended in this process it does not require more energy to set up a certain state for an entire region than for a smaller part (e.g., a wireless sending station is not loaded with an additional burden when you switch on your receiving set).

Critical Summary

In brief, Weiss has shown that the facts obtained regarding the centro-peripheral communication in an organism demand recognition that the motor end-organs possess the property of discriminatory reaction, and that excitations from the central nervous system are irradiated or broadcast to various end-organs rather than to one only. Thus a "message" emanating from the central nervous system along several neighboring paths reaches several muscles but is responded

to by only one muscle. In addition, he has shown that different excitations capable of innervating two or more muscles coordinately may be sent along one neuron. These two facts and their theoretical consequences Weiss maintains must be incorporated into the prevailing picture of nerve action.

In trying to envisage the operation of the centro-peripheral coordination Weiss has found it convenient to think of it in terms of the operation of a resonator system or a radio sending and receiving system. He cautions his reader, however, that his "resonance theory" is merely a provisional framework into which it has proved useful and stimulating to fit the existing facts while gathering new material.

In appraising Weiss's results it should be pointed out that they refer chiefly to the motor side of the nervous system, that they have been gathered from transplantation experiments which have disarranged Nature's scheme of things, that specific changes in the motor nerves have not been studied by the well-developed action-current technique, that they have been confined to amphibia (salamanders and toads) and consequently cannot be expected to apply in every detail toward an interpretation of the afferent nervous system in human beings. Since all nervous systems possess certain properties in common, however, it seems reasonable to admit that his conclusions would probably apply to the operation of nervous systems in general, and to the human nervous system in particular.

Weiss's results should be of especial interest to psychologists because of their nice conformity with facts discovered by other experimenters working from very different points of attack. Thus Lashley's theory of mass-action in the cortex of rats emphasizes the need of reorganizing our picture of nerve action along lines very like those developed by Weiss. The results of other investigators,[12] showing how the state of any part of the nervous system is influenced by other stimuli simultaneously occurring also support the "resonance theory." Bethe,[13] Henry Head, Bartlett, Piéron, and others[14] coincide with Weiss in stressing the importance of trying to

[12]For example, Hartmann (5) and Freeman (4).

[13]His article on "Plastizität und Zentrenlehre" (1) is a most valuable contribution to the problems under discussion. On pages 1198-1200 he discusses the resonance principle quite extensively. He is much more radical than Weiss.

[14]Weiss's findings fit in well with the Gestalt theory of psychophysiological stationary processes (Köhler, 7, 8). See also recent articles by Wiersma (31) and Dodge (3).

understand the operation of parts of the nervous system not in terms of differentiated structure but in terms of a dynamic organization of the whole system.

In view of the trend toward a general viewpoint which coincides with Weiss's theoretical conclusions we cannot afford to disregard his extensive array of well attested facts, stimulating theory, and significant criticisms of the prevailing orthodox theory of the functioning of the nervous system.

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LES CONCEPTIONS CHANGEANTES DANS LA PSYCHOLOGIE PHYSIOLOGIQUE

(Surtout à l'égard des Contributions de Paul Weiss)

(Résumé)

Cet article rapporte quelques-unes des contributions principales à la psychologie physiologiques faites par Paul Weiss. Weiss a transplanté les membres des amphibiens vivants en des organismes "hôtes" de la même espèce, et a observé comment le membre surnuméraire est arrivé à fonctionner d'une façon homologue avec le membre adjacent de l'hôte. On a constaté que la réinnervation des membres surnuméraires n'a pas compris une augmentation des fibres motrices qui sortent de la moelle épinière de l'organisme "hôte." Donc la réinnervation du membre surnuméraire a dû être accomplie seulement par la régénération des nerfs de l'organisme hôte après l'incision où l'on a planté le surnuméraire. Puisque l'on a fait des transplantations en diverses parties des corps des hôtes, ces données en somme prouvent la possibilité d'un comportement coordonné sans des connexions spécifiques neurales. (On a obtenu des résultats analogues avec les transplantations de muscles.) Ainsi, en contraste avec une théorie de connexions spécifiques, Weiss essaie d'expliquer la spécificité de la fonction au moyen de sa théorie de "résonance": un changement de condition dans le système nerveux fait sortir des réponses seulement de ces organes d'extrémité qui sont sensibles ("accordés") à cette modification spécifique. Un résumé critique indique de certaines limitations dans l'étendue possible de l'application de cette théorie.

DESILVA ET ELLIS

WECHSELNDE AUFFASSUNGEN IN PHYSIOLOGISCHER PSYCHOLOGIE (MIT BESONDERER BERUECKSICHTIGUNG DER BEITRAEGE VON PAUL WEISS)

(Referat)

Diese Abhandlung teilt einige der Hauptbeiträge zu physiologischer Psychologie von Paul Weiss mit. Weiss verpflanzte die Glieder lebender Amphibien auf Gastorganismen derselben Gattung und beobachtete, wie das überzählige Glied homolog mit dem angrenzenden Gastglied funktionierte. Neuinnervation der überzähligen Glieder bedeutete keine Zunahme der motorischen Fasern, die aus der Rückenmark des Gastorganismus herauskamen. Daher muss die Neuinnervation der überzähligen Glieder lediglich durch Nerven des Gastorganismus erzielt worden sein, die nach dem Einschnitt, in den das überzählige Glied gesetzt wurde, sich erneuert haben. Da die Verpflanzungen auf verschiedene Teile der Gastkörper gemacht wurden, beweist dieser Sachverhalt die Möglichkeit eines zusammenhängenden Verhaltens ohne spezifische Nervenverbindungen.

(Aehnliche Resultate bei Muskelverpflanzungen wurden erhalten.) Im Gegensatz zu einer Theorie der spezifischen Nervenverbindungen versucht Weiss, die Spezifizität der Funktion durch seine "Resonanztheorie" zu erklären: eine Veränderung des Zustandes in dem Nervensystem ruft Reaktionen nur von denjenigen Endorganen hervor, welche dieser spezifischen Veränderung empfänglich sind. Eine kritische Zusammenfassung weist auf gewisse Einschränkungen in dem möglichen Umfang der Anwendung dieser Theorie hin.

DeSilva und Ellis

ON THE DETECTION OF EMPHASIS IN SPOKEN SENTENCES BY MEANS OF VISUAL, TACTUAL, AND VISUAL-TACTUAL CUES*[1]

From the Department of Psychology of Northwestern University

DOROTHY MOSSMAN THOMPSON[2]

The contribution of tactual cues to the interpretation of speech has attained increasing significance since Dr. Robert H. Gault's preliminary experiments from 1922 to 1925 (2).

The problem under consideration here is part of the research program to discover the positive contributions of tactual stimuli to the interpretation of spoken language in the absence of auditory cues.

The purpose of this study was to discover abilities to detect emphasis in a sentence by means of visual, tactual, and visual-tactual stimulation. Preliminary to a study of the performance of hearing subjects, placed in conditions that eliminated auditory cues, we employed a deaf subject who is an experienced lip-reader. On the basis of no fewer than nine hundred judgments by each mode of stimulation, the experimenter compared accuracy in the detection of emphasis by the three methods respectively. Stimuli sentences were constructed so as to permit an analysis of sentence patterns into their components. Thus an examination of sentence or word structure discloses the presence or absence of factors favoring accurate detection of emphasis.

STATEMENT OF PROBLEM

Data derived from experimental observations comparing the ability of the finger-tips with that of the lip-reader's eye in the discrimination of speech patterns have definitely affirmed the statement that

*Recommended by Robert H. Gault, accepted for publication by Carl Murchison of the Editorial Board, and received in the Editorial Office, July 27, 1933.

[1]Abstract of a Master's thesis carried out under the general direction of Dr. Robert H. Gault in Northwestern University, June, 1932. The author expresses to him here sincere appreciation and to the Rev. Dr. Walter G. Williams, Miss Milla Ilieva, and Dr. Louis D. Goodfellow, whose co-operation and generous devotion of time are responsible for its successful completion.

[2]Case Worker Family Welfare Association of Milwaukee.

simultaneous visual-tactual stimulation offers a significant advantage over visual stimulation alone (3). It was to test further these findings and to aid in a survey of the specific points at which the advantage arises that the present investigation was undertaken. The problem is to compare the accuracy with which emphasis in a sentence can be localized by means of visual stimulation alone, tactual stimulation alone, and by simultaneous visual-tactual stimulation.

Supplementary to a quantitative study of the relative accuracy of the three modes of stimulation is a qualitative analysis of errors in identification based upon the vowel-consonantal composition of the emphasized word, its position in the sentence, and its gross pattern.

Emphasis, for the purpose of this investigation, is a summation of stress, pitch, and duration. Common usage offered the criterion as to the presence or absence of emphasis in the stimulus sentences. In order that the characteristics of emphasis in current speech practice might be retained in this experimental situation the guiding consideration in presenting the stimuli sentences was the question: "Is the specified word the emphatic word in the sentence?"

The experimenter is particularly indebted to Miss Dorothy Gold, formerly dramatic critic in Monmouth College, Monmouth, Illinois, for her recommendations in regard to patterns of pronunciation. With her assistance the experimenter devised gross patterns of speech for each sentence modification which, in the judgment of from five to ten disinterested individuals, convincingly conveyed the quality of emphasis. In each case she adopted for practice the pattern of speech that, by common agreement, was most effective.

Materials and Methods

A. Apparatus. The experimenter presented the stimuli for each of the three modes of stimulation from a sound-deadening room. The room is approximately 6½ x 4½ x 3½ feet, with walls heavily padded with strips of hair felt. The only access to the room is by a double-doored entry, the doors themselves carefully lined with sound-deadening material. The windows in the doors through which the speaker's face is seen in both the visual and the visual-tactual series are set at shoulder height in the two doors and lighted by an unshaded globe immediately above the center. Fastened on either side of the window were identical microphones, each having its own battery, in order additionally to sensitize the in-put circuit. In

cases of stimulation by tactual cues alone, the speaker's face was concealed from the subject by a cardboard shield fitted between the inner window and the microphones.

For tactual reception of stimuli the Gault-Teletactor[3] was suspended in a sound-deadening double box of cement which is a 34-inch cube, outside measurement. An opening is provided through the walls on one side sufficient in size to accommodate the forearm of the subject. The box is lined with several layers of cotton. Rubber sponge was used to pad the circumference of the hole so as to permit no escape of sound from the vibrating diaphragm of the Teletactor within the box. The weight of the double box is approximately 1500 pounds.

An additional precaution against the reception of auditory cues from the stimuli sentences was a sound barrier produced by a 60-cycle alternating current.[4] The subject fitted on the earphones securely after first packing his ears with several layers of cotton. The effect was to prohibit the reception of all auditory cues but not to tire the subject during short periods of time.

In the course of the experiment each subject was repeatedly tested for the reception of auditory cues through the air and through bone-conduction with consistently negative results.

B. Stimuli. Previous experiments with the Teletactor have disclosed the fact that apparatus for transmitting the energy of speech to the finger-tips conveys some consonantal-vowel combinations more advantageously than others. Thus a subject's success or failure in localizing the emphasis in a sentence might be a function of the instrument. The sentences adopted for use in this experiment, therefore, were not selected at random but rather were constructed to combine in various ways the major classifications of sounds. The authority for the classification we employed is the International Phonetic Alphabet as represented by Henry Sweet in his *Sounds of*

[3]The Teletactor now in use in the Laboratory was constructed by Mr. Albert H. Krause of the Physics Department in Northwestern University. It consists of a powerful receiver unit with a conical-shaped diaphragm. The receiver was energized by an amplifier (output 4 watts) which transmits frequencies between 200 and 2000 d.v. per second without distortion. (The whole apparatus is now—July, 1933—being rebuilt and improved.) Fletcher (1) states that frequencies above 2000 d.v. per second can be eliminated without destroying the intelligibility of speech.

[4]By means of a potentiometer we obtained a current of 45 milliamperes with a potential drop of 45 volts across the earphones.

English and Arthur Rippman in *Elements of Spoken English.* The classification is based upon the shape of the oral passage as the sound is uttered. The stimuli sentences may each be pronounced to bear four or five meanings depending upon which of four or five words in the sentence is emphasized. Each of the four or five words, in turn, is selected for its consonantal-vowel components. There are thirty sentences, each combining a particular vowel formation with a predominant consonant formation according to the following groups:

Vowels

I. Back oral vowels ("u" sounds)
II. Back oral vowels ("o" sounds)
III. Middle oral vowels ("a" sounds)
IV. Front oral vowels ("e" sounds)
V. Front oral vowels ("i" sounds)

Consonants

1. Dental continuants ("z," "s," "zh," "sh," "th," "r," "l")
2. Labial continuants ("v," "f," "w")
3. Front and back oral stops ("g," "k")
4. Dental oral stops ("d," "t")
5. Labial oral stops ("b," "p")
6. Nasal stops ("m," "n," "ng")[5]

The stimuli sentences are as follows:[6]

I. 1. The *truth* is the *school refuses* to *excuse* us.
2. The *wolves* are too *few* to *move now.*
3. Who *took* my *good cook-book* to *school*?
4. Who *could endure* if *truth* were *duty?*
5. You are *dubious* about *poor Bruno's beauty.*
6. The *woman's new tune* will *soon* be a *nuisance.*

II. 1. Her *claws* did *not scratch* the *doors.*
2. What! You *saw* the *fellow fall off* the *wall!*
3. *Call* the *dog* if the *coat* is *gone.*
4. The *doctor wrote* that she *ought* to *stop.*
5. *Bob hopes* the *boss* will *postpone* the *job.*
6. The *Lord* has *long known* my *innocence.*

III 1. My *father asked* which *dance* was *last.*
2. He *laughed after* the *white* one *arrived.*
3. The *guard asked* for the *cargo* of *glass.*
4. He *tried* to *hide* his *hard heart* after *that.*
5. The *public buys because* of my *prices.*
6. Does my *father demand* an *answer tonight?*

[5]Nasal sounds may be classified as either stops or continuants. Rippman prefers the former classification.

[6]The italicized words, now one, now another, bore emphasis. Roman numerals designate vowel groups. Arabic numerals refer to consonantal groups.

IV. 1. Tell *Jesse* to *sell* the *leather chairs.*
2. *When have* they *failed* to *face* the *facts?*
3. The *guest gave* the *cake* to the *beggar.*
4. The *guest said* his *friend made* him *wait.*
5. Let the *baby play* with the *paper bear.*
6. *Many friends mentioned* the *danger* to him *then.*

V. 1. Does he *think* we *killed* the *thieves?*
2. *Will* he *feel* very *weak* from the *swim?*
3. We will *begin* this *week* to *clean* the *kitchen.*
4. *Did* he *drink* the *tea* in the *street?*
5. Please *keep* the *people busy* until we *appear.*
6. He *means* to *meet* me near the *mountain.*

C. Experimental Procedure. The thirty sentences constructed for stimuli were presented to the subject in series of ten. Each modification of a sentence was typed on a card. Above the typed letters was a phonetic transcription as a guide to consistent pronunciation, and below were symbols to guide toward consistent expression—as pauses, holds, rise and fall of pitch, intensities, etc. The experimenter shuffled the cards of a series and presented them more than once in random order, listing them as they were pronounced. The subject had mimeographed copies of the sentences in his possession and a key showing what words in each sentence might bear emphasis. Thus, when he received a signal that a certain sentence was about to be read, he knew that one of four or five words would be emphasized. Instructions to the subject were to underline whichever one of the designated words he believed had been emphasized.

D. Subjects. The advantage of employing hearing rather than deaf subjects in this experiment lies in the fact that deaf persons of sufficient maturity to act as subjects have already acquired a degree of skill in lip-reading as against an approximate naïveté in the reception of tactual cues to speech and thus preclude a just comparison between the two modes of stimulation. As novices in both lip-reading and touch-reading, then, it would seem that hearing subjects permit more valid conclusions as to the relative accuracy in receiving emphasis by visual and by tactual stimulation.

The nature of the investigation set some restrictions upon the selection of subjects. Qualitative significance of the responses depended upon repeated presentation of the complete battery of stimulus sentences. Repetition was imperative, also, to compensate for the variations of presentation imposed by the use of the human voice. In view of the requirements of the experiment, therefore, it was de-

cided to collect a convincing number of judgments from each of four subjects who could devote several hours to the project, rather than to attempt to employ a greater number of subjects briefly.

Two of these were assistants in the laboratory. One was a professor. The assistants had had no more than an introduction to experiments employing the Teletactor prior to their participation in this work. The fourth was not associated with the Laboratory in any way. Previous to this study he had had no experience either in lip-reading or with the Teletactor. He had a minimum acquaintance with the program of the Laboratory.

Results and Interpretation

A comparison of accuracy in detecting emphasis in all sentences by each of the three modes of stimulation is shown in Table 1, A and B.

It is important to examine and compare the accuracy of the respective performances of the four subjects who were employed in

TABLE 1-A

Accuracy of Judgments Compared

Mode of stimulation	Total judgments	Total correct judgments	Mean of percentage correct judgments	Variability of distribution SD Dis.	Reliability of mean SD Av.
Visual	5080	3234	63.66%	25.6	.359
Tactual	5080	3484	68.58%	22.1	.3099
Visual-tactual	5080	4287	84.39%	17.9	.251

TABLE 1-B

Comparison of Differences and Their Reliability

Mode of stimulation	Actual difference	Sigma difference	Ratio	Chances per 1,000,000 that difference exists
Visual and tactual	3.5	1.04	3.36	9,992,100
Tactual and Visual - tactual	4.2	1.02	4.06	9,999,683
Visual and Visual - tactual	7.7	1.38	5.13	9,999,997

obtaining the 15,000 judgments that have been analyzed above. Table 2 shows a comparison of the mean percentage of correct judgments for each subject.

TABLE 2

COMPARISON OF MEAN PERCENTAGE OF CORRECT JUDGMENTS FOR EACH SUBJECT

Subject	Visual		Tactual		Visual-Tactual	
	Mean percentage of correct judgments	$S.D._{av.}$	Mean percentage of correct judgments	$S.D._{av.}$	Mean percentage of correct judgments	$S.D._{av.}$
L	43.4	21.92	59.3	21.12	79.6	17.15
G	55.3	22.72	60.0	23.36	78.1	21.45
W	77.6	20.70	78.5	18.75	91.7	11.20
M	78.4	18.20	76.5	16.50	88.1	14.90

The variability of means of percentage of correct judgments for each subject decreases from a range of 35.0 per cent in the case of visual stimulation to a range of 13.6 per cent in the visual-tactual situation, whereas the measures of variability progressively spread in range from a difference of 4.52 sigma in performance by visual stimulation to 10.25 sigma variation in visual-tactual performances. In other words, a study of the scores of the subjects individually indicates that the closest approximation of their measure of central tendency occurs in the visual-tactual situation, the mode of stimulation in which they achieved greatest accuracy, and that the least disagreement in their measures of variability occurs in the visual situation, the mode of stimulation in which their performance was least accurate. The fact deserves particular mention that the most accurate performance in respect both to central tendency and to variability for modes of stimulation involving tactual cues occurs in the case of Subject W. He served in the experiment with only sufficient knowledge of its significance to motivate him to maximum concentration throughout the several hours of his time it required. His fields of activity are entirely foreign to the program of experimentation involved in a survey of the contribution of tactual cues to the interpretation of language by the deaf. As a control subject his scores would signify again the high reliability of the findings in regard to the positive contribution of tactual cues in the detection of emphasis in a sentence.

Evidence presented above definitely establishes the original hypothe-

TABLE 3A
SENTENCES RANKING HIGHEST IN ACCURACY

	Percentage correct	Components: Vowel	Components: Consonantal
a. Visual Stimulation			
Wolves few move now	70.0	back vowels "u" sounds	labial continuants
Bob hopes boss postpone job	72.5	back vowels "o" sounds	labial stops
Public buys because prices	72.0	middle vowels "a" sounds	labial stops
My demand answer tonight	77.0	middle vowels "a" sounds	nasal stops
Guest said friend made wait	73.0	front vowels "e" sounds	dental stops
Baby play paper bear	69.0	front vowels "e" sounds	labial stops
Means meet near mountain	69.0	front vowels "i" sounds	nasal stops
b. Tactual Stimulation			
Truth school refuses excuse	73.7	back vowels "u" sounds	dental continuants
Woman's new tune soon nuisance	77.5	back vowels "u" sounds	nasal stops
Claws not scratch doors	76.0	back vowels "o" sounds	dental continuants
Father asked dance last	73.7	middle vowels "a" sounds	dental continuants
Jesse sell leather chairs	76.0	front vowels "e" sounds	dental continuants
Guest said friend made wait	77.5	front vowels "e" sounds	dental stops
Think we killed thieves	75.0	front vowels "i" sounds	dental continuants
Did drink tea street	73.7	front vowels "i" sounds	dental stops
c. Visual-Tactual Stimulation			
Claws not scratch doors	93.7	back vowels "o" sounds	dental continuants
Saw fellow fall off wall	91.0	back vowels "o" sounds	labial continuants
Bob hopes boss postpone job	91.0	back vowels "o" sounds	labial stops
Public buys because prices	93.7	middle vowels "a" sounds	labial stops
My demand answer tonight	91.0	middle vowels "a" sounds	nasal stops
Guest gave cake beggar	91.8	front vowels "e" sounds	back & front stops
Guest said friend made wait	98.7	front vowels "e" sounds	dental stops

TABLE 3B

SENTENCES RANKING LOWEST IN ACCURACY

	Percentage correct	Components: Vowel	Components: Consonantal
a. Visual Stimulation			
Took good cook-book school	45.0	back vowels "u" sounds	back & front stops
Dubious poor Bruno's beauty	58.0	back vowels "u" sounds	labial stops
Father asked dance last	58.7	middle vowels "a" sounds	dental continuants
Many friends mentioned danger than	57.5	front vowels "e" sounds	nasal stops
Think we killed thieves	53.7	front vowels "i" sounds	dental continuants
Did drink tea street	56.0	front vowels "i" sounds	dental stops
Keep people busy appear	58.7	front vowels "i" sounds	labial stops
b. Tactual Stimulation			
Saw fellow fall off wall	61.0	back vowels "o" sounds	labial continuants
Doctor wrote ought stop	65.0	back vowels "o" sounds	dental stops
Tried hide hard heart that	66.5	middle vowels "a" sounds	dental stops
Public buys because prices	62.0	middle vowels "a" sounds	labial stops
When have failed face facts	57.5	front vowels "e" sounds	labial continuants
Begin week clean kitchen	60.0	front vowels "e" sounds	back & front stops
c. Visual-Tactual Stimulation			
Truth school refuses excuse	80.6	back vowels "u" sounds	dental continuants
Took good cook-book school	73.0	back vowels "u" sounds	back & front stops
Woman's new tune soon nuisance	81.0	back vowels "u" sounds	nasal stops
Call dog coat gone	78.7	back vowels "o" sounds	back & front stops
Lord long known innocence	77.5	back vowels "o" sounds	nasal stops
Jesse sell leather chairs	79.3	front vowels "e" sounds	dental continuants

sis that "when the highly specialized ear ceases to function, the organ

of touch may serve as a partial substitute" (2). It is possible that further examination of the conditions in which emphasis was detected or confused may lead to some understanding of what factors in speech favor or inhibit the detection of emphasis by each of the three modes of stimulation outlined above. A careful control and record of the varying characteristics of the stimuli sentences were kept in the hope that they would disclose the presence or absence of common factors in cases of high and of low accuracy.

In Table 3, A and B, appear the sentences that ranked highest and lowest respectively in the percentage of accuracy with which their emphasized words were detected.

A study of Table 3 discloses a distinct preference for continuant consonantal components among sentences ranking highest in accuracy by tactual stimulation and a preponderance of stop consonantal components among sentences ranking lowest in accuracy by that mode of stimulation. Five of the eight sentences employed dental continuants in the former case, and four of the six sentences employed stops in the latter case. Four sentences ranking first in the visual-tactual situation occurred also among sentences of highest accuracy by visual stimulation. The preponderance of stop consonantal components occurs in both situations.

The Question of Learning

It is possible to conceive of a subject improving his accuracy by a process of learning to recognize each of the 127 sentence patterns after the first several presentations. Should a memory process be responsible for the high record of accuracy, a large number of judgments would signify little in regard to a comparison of abilities to detect emphasis in a sentence by means of the three modes of stimulation. Positing as evidence for learning any series of ten judgments whose errors occurring in the first three judgments are corrected in the last three judgments, the original records of the subjects were re-examined. In the 508 series by each mode of stimulation there are 85, 119, and 100 cases, respectively, for visual, tactual, and visual-tactual stimulation in which the correction of original confusions might be interpreted as a product of a learning process. Chance would permit such a situation to occur in 127 cases. At the end of experimentation with each series of 10 stimulus sentences the experimenter gave each subject an additional test of learning. One modification of each stimulus sentence was presented

with no signal to designate which sentence was being spoken. The subject was instructed to make a judgment as to which of the 10 stimulus sentences in the series was being read, and which word in the sentence bore the emphasis. While the identifications of sentences achieved an accuracy considerably more than a chance allowance of 12, i.e., 97, 34, and 102 cases out of 120 possibilities, the accurate detection of emphasis, 55, 49, and 71 correct judgments for the three modes of stimulation, respectively, fell a little above the chance probability of 31. Whatever accuracy is achieved in this phase of the experiment, it is uncertain whether a learning process or intrinsic factors of the sentence pattern is the more responsible for a record superior to one controlled only by chance factors. Performance on each sentence modification as interpreted from the data would lead one to attribute significance to intrinsic factors in the sentence pattern.

Summary and Conclusions

In the case of high accuracy by each mode of stimulation the emphasis-bearing word most frequently occurred in the first part of the sentence. In contrast the majority of the emphasis-bearing words least accurately detected were near the end of the sentence.

A study of the grand total of 5080 judgments as to the location of emphasis in a sentence by means of each of three modes of stimulation, visual, tactual, and visual-tactual, when hearing subjects were employed in a situation in which hearing was eliminated, shows conclusively that accuracy increases in the order in which the situations were named above. The *means* of percentage of correct judgments increase from 63.6 ± 14 in the visual situation, to 68.58 ± 16.5 in the tactual situation, and 84.39 ± 13.4 in the visual-tactual situation. The difference between accuracy in the visual and tactual situations and between tactual and visual-tactual situations was found to be highly reliable in all experimental conditions.

An examination of the components of stimuli by means of which the highest and lowest accuracies, respectively, were attained, discloses the following tendencies:

In the visual and the visual-tactual situations back vowels outnumbered the front vowels at ratios of 9 to 2 and 12 to 3 respectively. Whereas dental sounds prevailed as consonantal components of the words tactually detected with greatest accuracy, labial sounds occurred most frequently in the visual-tactual situation. Among

words least accurately detected dental sounds predominated as consonantal components in visual and visual-tactual modes of stimulaton. Stops prevailed among words least accurately detected by means of tactual stimulation.

In confusions that occurred at greater than chance frequencies in both the visual and visual-tactual situations, back vowels were the prevailing vowel components, and dental sounds the prevailing consonantal components, of sentences in which the confusions were consistent. Front vowels predominated in the tactual situation. While the stimulus word occurred near the beginning of the sentence in the majority of cases for visual and tactual stimulations the word consistently substituted was most frequently near the end of the sentence.

Back vowel and stop and dental consonantal components characterized word patterns *most frequently and accurately detected by visual cues,* and they usually occurred near the beginning of the sentence. Emphasized words near the end of the sentence and composed of back vowel and dental consonantal sounds were consistently difficult to detect. *In the tactual situation dental and continuant consonantal components predominated among words frequently* detected whereas *stop and front vowel components appeared in words causing the most confusion.* Early occurrence in the sentence again favored accurate detection. Although words accurately detected by *visual-tactual cues shared characteristics of both visual and tactual modes, the components favoring ease of detection by visual cues predominated.*

In few cases are the common characteristics of words consistently detected or confused dominant enough to be conclusive. However, the findings indicate that accuracy in the detecting of emphasis in a sentence may be modified by a change in the placement of the word in the sentence and in its vowel-consonantal components.

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SUR LA DÉCOUVERTE DE L'ACCENTUATION DANS LES PHRASES PARLÉES AU MOYEN DE SUGGESTIONS VISUELLES, TACTILES ET VISUELLES-TACTILES

(Résumé)

C'est une partie d'un programme de recherche dirigé par le docteur Robert H. Gault à l'université Northwestern. Le but est en somme de déterminer l'étendue de l'utilité du sens tactile comme aide de l'interprétation de la parole et comme méthode de développer le langage vocal.

Cette enquête spéciale a le but de découvrir la précision relative de la vision (comme dans la lecture des lèvres), du sens du toucher, et des sens du toucher et de la vision combinés dans la découverte et la localisation des mots accentués dans la parole.

Trente phrases ont servi comme stimuli. On a accentué un de quatre ou de cinq mots, choisis sur la base de combinaisons de voyelles et de consonnes.

On les a présentées premièrement, comme aux sourds pour la lecture des lèvres; deuxièmement, à un doigt à sentir comme complexes de formes de vibration; troisièmement, à l'oeil et au doigt simultanément. Dans la deuxième méthode et la troisième on s'est servi du Teletactor-Gault pour la communication au doigt.

L'analyse de 5080 jugements montre que la précision s'accroît en l'ordre suivant de présentation: visuel, tactile, visuel-tactile.

Cela soutient la conclusion d'une enquête antérieure par le docteur Gault, que l'interprétation de la parole par la stimulation visuelle-tactile est supérieure à l'interprétation dans la situation usuelle de lecture des lèvres.

THOMPSON

UEBER DIE AUFDECKUNG DER BETONUNG IN GESPROCHENEN SAETZEN VERMITTELST GESICHTS-, TAST- UND GESICHTS-TASTWINKEN

(Referat)

Diese Untersuchung ist ein Teil eines Forschungsprogramms, das von Dr. Robert H. Gault an der Northwestern Universität geleitet wird. Der Versuch wurde gemacht, den Umfang der Brauchbarkeit des Tastsinnes als Hilfe zur Deutung der Sprache und Entwicklung der Stimmsprache festzustellen.

Diese Unterschung zielt darauf hin, die relativ Genauigkeit des Gesichts (wie beim Lippenlesen), des Tastsinnes, und des Tast- und Gesichtsinnes zusammen bei der Aufdeckung und Lokalisierung betonter Wörter beim Sprechen klarzulegen.

Dreissig Sätze wurden angewendet. Eins aus vier oder fünf Wörtern wurde betont, das auf Grund der Vokal-Konsonantenverbindungen gewählt wurde.

Die Sätze wurden dargeboten: erstens als zu den Tauben zum Lippenablesen; zweitens dem Finger als Komplexe von vibrierenden Gestalten; drittens dem Auge und dem Finger zugleich. In dem zweiten und dritten Versuche wurde der Gault-Tastapparat (Gault Teletactor) zum Reizen des Fingers verwendet.

Die Untersuchung von 5080 Urteilen zeigt, dass die Genauigkeit in folgender Ordnung zunimmt: Gesicht, Tast, Gesicht-Tast.

Dies unterstützt den Schluss einer früheren Untersuchung von Dr. Gault, dass die Deutung der Sprache durch Gesicht-Tastreizung der Deutung in der gewöhnlichen Lippenablesensituation überlegen ist.

THOMPSON

MOTOR SKILLS: THE VALIDITY OF SERIAL MOTOR TESTS FOR PREDICTING TYPEWRITING PROFICIENCY*[1]

From the Psychology Laboratory of the University of Oregon

ROBERT Y. WALKER AND RAYMOND D. ADAMS

INTRODUCTION

The hypotheses of motor aptitudes assume that there are certain basic coordinations, measurable in a short period of time, which determine the degree of skill likely to be attained through longer training in more complex performances. This paper presents an experimental evaluation of serial motor tests as bases for selecting students most likely to succeed in learning to type. Knowing the predictive efficiencies of these motor tests we should then have an indication of the relative emphasis to be placed upon the selection of personnel for this work and upon studies for the improvement of training and work methods, or upon other phases of the personnel approach—the use of verbal tests or personality ratings.

In a review of the studies in this field up to the present R. H. Seashore (8) shows a preponderance of evidence against the existence of either a "general motor ability" or of more specific coordinations which might be considered to be fundamental abilities or aptitudes. For example, Muscio (4) concludes that there is no "motor type" and that such terms as "motor dexterity" or "practical ability" are misleading because the measure of a person's performance on one test is no positive indicator of his ability on another test.

Another hypothesis, that individual differences in motor performances are determined by simple variables such as reaction time, steadiness, and speed of reflex movement is also refuted by experi-

*Recommended by Dr. R. H. Seashore, accepted for publication by Carl Murchison of the Editorial Board, and received in the Editorial Office, January 27, 1933.

[1]These studies are a part of a series on motor skills from the University of Oregon Psychology Laboratory. The writers are indebted to Mr. J. F. Elton and to the typing instructors at the Commerce High School of Portland, Oregon, for cooperation in securing the data.

mental evidence. For instance, numerous studies have shown that speed in "simple reaction" was not related to speed of tapping, even when the same hand and the same apparatus were used in both tests. Neither did simple reaction time correlate with skill in any more complex serial performances, such as the tests of the Stanford Motor Skills Unit.

In contrast to the studies thus far presented, Spaeth and Dunham (10) report success in selecting rifle marksmen from scores on a simple "steadiness" test, as shown by a correlation of $.61 \pm .11$. Since these results are incongruous with the findings of other investigators and since chance variations of correlations may occur in spite of reliably controlled conditions, the junior writer is subjecting this study to an experimental verification.

A third hypothesis suggests that individuals are endowed with certain "basic motor capacities," combinations of which determine the ability to perform certain complex tasks. Among these basic motor capacities may be listed speed of tapping, serial discrimination, eye-hand coordination in following targets, postural stability, etc. These "basic motor capacities" were believed to be serial or continuous coordinations, largely innate and therefore subject to but little improvement by practice, and basic to more complex coordinations of the same general type. Unfortunately the hypothesis breaks down at almost every point. First, some of the examples cited, e.g., serial discrimination, are greatly changed by practice. Secondly, even slight changes such as a shift from visual to auditory stimuli produce marked changes in individual scores in the tests, which means that there must either be a very large number of "basic" capacities, or else that sensory and other factors are of great importance in "motor" test scores. Finally, they have never been definitely shown to be "basic" to any other skilled acts, even where there seemed to be some logical basis. McCollom (3) found that while several tapping tests would agree among themselves if all involved movement in a single plane, a change to the slightly more complex movement of tapping alternately on two plates gave very different results. The two-plate test was related to a three-plate test and to a peg board which was especially designed to involve very similar movements. However, these two "group factors" were so narrow in scope that neither set of these tapping tests helped to predict the speed of more complex tests which also seemed to be somewhat similar in nature.

S. H. Seashore (9) used six motor tests of the Stanford Motor Skills Unit which were generally accepted as examples of basic motor capacities in a study of complex machine skills. At the Jantzen Knitting Mills in Portland, all 60 men worked on the same type of winding machine under good working conditions and were paid according to output of yarn wound. "Correlations of the separate motor tests with the criterion of work done ranged from —.07 for the Koerth pursuit rotor to .36 for rotary [drill] speed, with an average of +.16. The correlation between the unit as a whole and the criterion was +.14±.10. Considering the [objectivity and] high reliabilities of both the tests and criterion this non-significant correlation may be offered as definite evidence against the hypothesis of a group of basic motor skills as determinants of practical machine skills, and as pointing towards a theory of highly specific motor skills."

That individual differences in motor performances are ascribable to differences in training is a tentative explanation, but as yet there has been relatively little research to determine its validity. Another explanation which has been relatively neglected by most writers is that the work method or technique which one employs in the performance of a strange task is important and it may be partly determined by previous experience and training—those individuals choosing the proper methods becoming the most proficient workers. Gilbreth (2) developed a rather unique method of analyzing the movements in complex skills by an instrument of his own design known as the stereochronocyclegraph. Although he was quite successful, his untimely death has delayed a wider application of his technique.

In summarizing this theoretical discussion of motor skills, we are forced to conclude that the validity of any of the above explanations of individual differences is extremely uncertain. The literature does not substantiate the presence of any general factor but indicates that motor skills are highly specific. Even though the evidence so far is strongly negative to the idea that it is possible to predict practical skills from motor tests, the widespread belief in this procedure makes it advisable to secure critical evidence from actual tryouts on a number of representative practical skills.

Since typing is generally recognized as a fine motor skill, and since both typing and simple motor skills lend themselves to reliable measurement, the present study should be critical to the general problems

of the field. Even though this study, as well as the one by S. H. Seashore (9), tends to refute the "aptitude hypothesis," a number of further studies in other practical fields should be made before any sweeping conclusion against these hypotheses is accepted.

Turning now to previous studies more definitely related to the prediction of typing, we find that Steadman (11) tried out a battery of "mechanical ability" tests for prediction of typing. The highest correlation which she obtained was with the MacQuarrie test of "mechanical ability," a paper and pencil battery, which gave an $r = .38 \pm .10$ with speed of typing; $r = .09 \pm .11$ with accuracy. However, the small number of cases, only 36 subjects being tested, together with the possibility of chance variations leaves the significance of this study rather uncertain.

To determine the importance of "voluntary motor ability"[2] in typing Book (1), at the University of Indiana, revised the Bryan Test of Voluntary Motor Ability and tested a group of 48 state typing champions among whom were five ex-champions and one public speed demonstrator. A special apparatus consisting of a telegraph key connected electrically with a Hollerith Electric Speed Counter was used to record the number of movements made in five seconds. The test was also given to 55 students studying typing at Indiana University and the relationship between their "voluntary motor ability" and typing grades was computed.

Book reported (1) that the world's champion typists were vastly superior to the average individual of the same age; (2) that the increased ability found in these groups is not developed by practice because the beginners group possessed as much voluntary ability as the advanced students; (3) that the superior typing students were slightly superior in voluntary motor ability to the poor students, at least the difference was distinct enough to be a prediction of later typing grades.

To explain the above results one has recourse to two possible interpretations: (1) Individual differences in "voluntary motor ability" may be indices of individual differences in typing ability, but

[2]This merely refers to the tapping speeds of forearm, wrist, fingers, etc. In the light of the previous studies reviewed in this paper the term "voluntary motor ability" is seen to be a misnomer because the tapping tests are unrelated to other motor tests and therefore cannot be representative of the whole field.

if so, the importance of a limitation in tapping speed would probably not appear except in the very highest rates of typing speeds, and the rates of these "champions" were three or four times as great as the ordinary typing students. (2) Those individuals who have received intensive training in typing over long periods of time may develop a superiority in "voluntary motor ability." Book believes that he has excluded the possibility of transference of training by showing that the poor beginner students have less "voluntary motor ability" than the superior students. However, it is possible that long periods of training at high speeds such as the professional typist receives would increase the ability to tap, even though there is no observable transfer for beginning typists and amateurs who never approach their tapping speed in typing. Furthermore, an examination of the differences in average tapping speeds of A, B, C, D, etc., typing students, cited by Book, shows that the amounts of group differences are so small as to be insignificant in comparison with the ordinary range of tapping speeds.

Tuttle (12) selected a battery of six psychological tests including tests of motor action, attention, and substitution, which he believed would measure some of the processes involved in typewriting. From his results Tuttle predicted that those who made a mark of 80 or better on this battery of tests might become expert typists; that those who scored below 75-79 would probably be average typists; and that those who scored below 75 would probably fail or become poor typists. However, the range of scores between failure and success, 75-80, is certainly too narrow to afford a high degree of accuracy in individual predictions. Since there were only twenty subjects used and no reliabilities reported, the evidence presented here does not dispose one to make a decisive judgment as to the validity of the "aptitude hypothesis."

Robinson (6) used a battery of paper and pencil tests and a serial action test involving the use of four keys on a typewriter in an attempt to predict success in typewriting. He obtained correlations ranging from $r = .06$ to $r = .35$ between these tests and achievement in typewriting, correlations which are too low to be of predictive value.

Ohmann (5) tested 225 typing students with a battery of tests among which was a measure of tapping speed. He obtained a correlation of .04 between the scores made on this battery of tests and

the typing ability as measured by the Blackstone Stenographic Proficiency Test, Form D (a method by which the rate of typing is determined by multiplying the gross number of strokes by 10 and dividing by 10 ÷ number of errors). This correlation would indicate that tapping is of no value as a prognostic test for typewriting proficiency. Here, too, we must be cautious in accepting this result as critical to the "aptitude hypothesis" because the writer neglected to report the reliabilities of his measures.

Seashore (8) as a part of a larger experiment tested 50 men on the Stanford Motor Skills Unit, and then rated them for the amount of training in typewriting which they had received. The correlation was slightly positive but was of no predictive value. This study is open to criticism in that it does not provide for a more objective criterion measure. Even though there was no correspondence between the amount of training in typewriting and serial discrimination (one of the tests most closely resembling typing) it must be remembered that these students had only moderate amounts of training. This does not preclude the possibility of a definite transfer in cases of intensive training.

Articles by Bills, Chapman, and others were reviewed but are not discussed in the text of this study, because they were not believed critical to this particular problem.

The general status of the application of motor tests to the prediction of typewriting proficiency has been given. The evidence against any general motor ability could be supplemented but it is felt that this material is representative. Two papers (Book and Tuttle) support the possibility of predicting proficiency by tapping ability; but two other experiments show that the tapping test is of little value in estimating future typing proficiency.

Since several of the tests in the Stanford Motor Skills Unit had not been used to predict typing and since previous experimenters present conflicting evidence, it was believed desirable to investigate the possible predictive value of the Stanford Motor Skills Unit for typewriting proficiency. This problem is also of theoretical significance in analyzing the determinants of the large range of observed individual differences in practical motor skills.

As a critical evaluation of the aptitude hypotheses, the senior author employed a battery of six serial motor tests comprising the Stanford Motor Skills Unit in an attempt to predict the degree of skill which

would be attained during seven months training in typing. The degree of success in predicting typing efficiency should be a good indication of the degree of validity of the aptitude hypothesis (for fine motor skills). If prediction is not successful, then other explanations of the acquisition of skill must be investigated.

Seventy male students ranging from 15 to 18 years in age and all enrolled in beginning classes of typewriting in the High School of Commerce, Portland, Oregon, were selected as subjects in this experiment. These students were trained in regular classes for seven months by standard typing methods at the end of which time they were given two speed tests of ten minutes each on successive days. The criteria of the typing speed were (1) the raw scores or number of strokes, and (2) the weighted scores which are corrected for errors according to the International Contest rules which deduct 50 strokes for each error.

During the first two weeks of school each student was tested on the Stanford Motor Skills Unit (7), for which the apparatus and directions have already been published. This battery of tests is made up of six tests, namely: (1) tapping on a telegraph key, (2) tapping a key in time with a rhythm pattern, (3) following with a pointer a small target which revolved at high speed, (4) turning a hand drill for 10 seconds, (5) serial discrimination of numbers on a four choice apparatus, and (6) packing spools in a box. These six tests were selected by Seashore to represent major types of *fine* muscle coordinations, chiefly manual, as opposed to large muscle athletic tests. In previous studies they have been found to have fairly high reliabilities, $r_{½-½}$=.75 to .94, with low intercorrelations between tests.

Since fatigue might have a deleterious effect on the reliability of a motor test, a *cycle* plan of testing was arranged, whereby a test requiring a maximum amount of effort in one muscle group would be followed by a test requiring less effort in that particular muscle group. A cycle consisted of approximately five minutes' work on each test in rotation. Three cycles were given, the total time being approximately one hour and thirty-five minutes, and the final score was the total of the second and third cycles for each test.

To maintain constant conditions standard instructions and demonstrations were given each of the subjects and each one was permitted to try the test to insure thorough understanding. In the case of

the motor tests the experimenters aroused interest by the competitive situation of having two students taking the tests at the same time. There was a constant change of task during the half hour with no time for loitering; only the necessary rest periods between tests were allowed. The Pearson product moment method was used for all the correlations. Although the usual classroom competition for grades was at first considered to be an adequate incentive for typing scores, on reconsideration we became doubtful as to whether it was sufficient.

Results

It was observed throughout the typing tests that the students were extremely cautious. Even the better typists preferred to go slow and avoid the penalty of 50 strokes for each error.

Several of the students seemed rather diffident in their typing test. The most likely explanation is that typing in this school was compulsory and some of the students are not interested in or even dislike typing.

Motivation on the motor skills set was unusually good, all the subjects evincing interest in the apparatus and in the scores they made.

The reliabilities of the six different tests, similar to those reported in previous studies, were obtained by correlating the scores of cycle two against those of cycle three. These are given in Table 1 (r's not stepped up by Brown Spearman formula).

TABLE 1
Reliability of Motor Tests
N = 70

	r	P.E.
Koerth Pursuit Rotor	.93	.01
Motor Rhythm	.78	.03
Serial Discrimeter	.89	.01
Speed of Tapping	.86	.02
Brown Spool Packer	.74	.03
Speed Rotor	.83	.02

Using the totals of cycles two and three for the working score, the intercorrelations of the six different tests are shown in Table 2. While some of these intercorrelations are fairly high, a comparison

TABLE 2
INTERCORRELATIONS OF MOTOR TESTS
N = 70

		I	II	III	IV	V
I	Koerth Pursuit Rotor					
II	Motor Rhythm	.29				
III	Serial Discrimeter	.29	.42			
IV	Speed of Tapping	.28	.54	.19		
V	Brown Spool Packer	.44	.32	.34	.35	
VI	Speed Rotor	.40	.27	.06	.46	.43

with other data on the same tests shows these to be chance fluctuations about a mean value of approximately +.25.

The reliabilities for typewriting were based on the test papers of 86 students, not all of whom were given the tests of the Stanford Motor Skills Unit. The reliabilities for raw scores and weighted scores were determined from tests taken on successive days. The tests were equal in difficulty and were given under identical conditions. The uncorrected reliability coefficients were found to be: for raw scores on the two tests, $r = .89$, for the weighted score on the two tests, $r = .60$, and for number of errors, $r = .28$. The lower reliability of the weighted score is probably explained by the still lower reliability of the errors which are heavily weighted in obtaining the corrected score. As seen in Table 3, the only motor test which showed a statistically significant correlation with typewriting was tapping; and even here the result was not the same for "weighted" and unweighted typing scores. The coefficients of correlation of the composite score and the separate motor tests against the "raw score" and the "weighted score" in typewriting are given in Table 3.

TABLE 3
CORRELATIONS OF MOTOR TESTS SCORES WITH TYPING SCORES
N = 70

Test	Typing Raw score		Typing Weighted score	
	r	P.E.	*r*	P.E.
Composite score (sum of standard score for all tests)	.35	.09	.15	.10
Koerth Pursuit Rotor	.07	.10	.10	.10
Motor Rhythm	.29	.09	.00	.10
Serial Discrimeter	.27	.09	.11	.10
Speed of Tapping	.28	.09	*.62*	.06
Brown Spool Packer	.27	.09	.15	.10
Miles Motility Rotor	.35	.09	.18	.10

Since this correlation of .62 between speed of tapping and "weighted score" seemed rather incongruous with the other results in this test as well as with results obtained by other investigators in the field, it was decided that a verification should be undertaken during the following year. Consequently in 1931 another group of 62 students enrolled in the beginning typing course at Commerce High School, Portland, Oregon, was tested by Adams for speed of tapping and correlations were computed between these scores and their typing scores. The previous experimental situation was duplicated as nearly as possible.

The results of this recheck strongly suggest that the single significant correlation of .62 from the first experiment was due primarily to chance variation. The correlations between scores on the tapping test and typing proficiency as determined by the three typing tests (designated I, II, and III) are given in Table 4.

TABLE 4

CORRELATION OF TAPPING AND TYPING TESTS ON RECHECK STUDY

Typing I (taken in November)	"raw" score	r .093
Typing I (taken in November)	"weighted" score	r .176
Typing II (taken in April)	"raw" score	r .127
Typing II (taken in April)	"weighted" score	r .143
Typing III (taken in June)	"raw" score	r .131
Typing III (taken in June)	"weighted" score	r .030

The reliabilities of the tapping test, determined by correlating the second trials against third trials were found to be $r_{1/2\text{-}1/2}=.85 \pm .02$. The reliability of the second typing test against the third, for "raw" scores was $r_{1/2\text{-}1/2}=.82\pm.03$ and for weighted scores $r_{1/2\text{-}1/2}=.648\pm.05$ (uncorrected).

Since there is some doubt as to the adequacy of the motivation in typing and since the group average on the typing tests was quite low, regardless of the six-month training period, one could easily entertain the possibility of a poor selection of subjects obscuring the true relationship between the experimental variables. For this reason we decided, in collating the results, to compare the scores of the best typists and best tappers. Table 5 summarizes the results for the three fastest and three slowest typists of the group tested by Adams.

Prior to the completion of this study several difficulties were encountered of which the following were judged as possible sources of error. The typing tests used provide such a heavy penalty for errors

TABLE 5

Subject No.	Raw typing score (strokes per minute)	Weighted typing score (words per minute)	Tapping score (rate per sec.)
35	193	38	7
45	194	34	7.3
22	184	35	6.5
Average, three best typists	190	35.6	6.9
42	100	8	7.2
34	105	8	7
14	105	8	6.3
Average, three slowest typists	103.3	8	6.8
Average entire group of 61	140.46	23.14	6.9

that an overcautious attitude is established which results in slower movement.

On the recheck some difficulty was encountered while testing. A six-volt battery was used in the circuit and might not have been strong enough to record some of the finer movements. Nevertheless, a comparison with other studies showed that some fairly high tapping scores were made. This error would, however, be a constant for the group.

Both the criterion measures and the tapping measures were for periods of short duration and might not have been sufficiently long to get a true measure of the subject's ability. However, as judged by the usual statistical standards, they were sufficiently reliable for the uses of the experiment.

Conclusions

We may sum up the results of this experiment as follows:

1. The separate tests of the Stanford Motor Skills Unit all have fairly high reliabilities as shown in other studies, and again in this paper (r½-½ from .74 to .93, not stepped up).

2. The reliabilities of this age group on the motor tests are very similar to those of the university group who are more highly selected, and who are about four years older.

3. The present method of grading typewriting papers on the basis established by the International Speed Tests is not as reliable

($r = .60$) as grading them on the basis of gross number of strokes ($r = .89$). This is probably due to the large weighting of the errors, which are themselves very unreliable ($r = .28$).

4. There is no significant relationship between the subject's score attained in the battery as a whole and the proficiency he may attain in typewriting (low correlations of $r = .35 \pm .09$ and $r = .15 \pm .10$ between composite score and the "raw" and "weighted" typing scores respectively). From this it is inferred that the aptitude hypothesis of individual differences in motor skills is of little importance for the prediction of *ordinary* typing speeds.

5. The higher tapping speeds of "champion" typists reported by Book may be due either to selection through limitations in tapping speed which become important at very high typing speeds, or may be due to transfer of training from high speed practice over a longer period of time.

It would be advisable to find a more adequate criterion of typing ability than the one used in this experiment. For instance, it would probably be advantageous to take the average of a large number of speed tests taken as daily assignments, and to try out various smaller penalties for errors. Both of these factors should produce more reliable measures.

A study should also be made of the pedagogical methods used to develop accuracy in the students, e.g., the present method of requiring perfect copies in daily assignments may unreasonably retard the progress of the student.

Since typing was compulsory in this school the experiment should be repeated under conditions which provide for more adequate motivation, and perhaps for longer periods of training.

An analysis should be made of the work methods used in typing to determine whether the students who made the best typing scores were using the best methods. This suggests the larger problem of the importance of training procedures and the adoption of efficient work methods as causal factors of individual differences in motor performances.

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LES HABILETÉS MOTRICES: LA VALEUR DES TESTS MOTEURS EN SÉRIE POUR PRÉDIRE LA CAPACITÉ D'ÉCRIRE À LA MACHINE

(Résumé)

Un résumé des écritures montre que les différences individuelles dans une bonne exécution motrice sont généralement attribuées à (1) une fonction motrice générale, (2) nombre de variables simples, (3) nombre de capacités motrices fondamentales, ou (4) aux différences de l'entraînement. Aucune de ces hypothéses ne semble compatible avec les résultats de cette étude et ceux des études antérieures.

On a testé soixante-dix étudiantes mâles, tous des commençants en écriture à la machine, avec six tests du Stanford Motor Skills Unit (Koerth Pursuit Rotor, Motor Rhythm, Serial Discrimeter, Tapping, Brown Spool Packer, et Speed Rotor). Après un entraînement de sept mois à écrire à la machine, on a fait subir des tests étalons pour l'écriture à la machine.

Sur les tests moteurs les constances ont varié de 0,74 à 0,93, tandis que sur les tests de l'écriture les constances ont été de 0,89 pour le nombre total de coups par minute, et de 0,6 pour le nombre des mots par minute après une correction pour les erreurs. Comme l'on a rapporté dans d'autres études avec le Motor Skills Unit, les intercorrélations ont donné une moyenne d'environ 0,25. La seule corrélation de signifiance statistique entre les résultats des tests moteurs et ceux de l'écriture à la machine a été le coefficient de 0,62 entre les résultats du "tapping" et ceux de l'écriture pesés.

Un nouveau test subséquent fait sur soixante autres étudiants de l'écriture à la machine a indiqué que le "tapping" ne prédit pas l'habiléte à écrire à la machine, les *r*'s variant de —0,1 à —0,176. La corrélation antérieure de 0,62 aura été due au grand pesage des erreurs, où la constance entre l'un test et l'autre n'a été que de 0,28.

On met sérieusement en question les hypothèses des aptitudes, ce qui suggère la nécessité des approches différentes telles que des méthodes efficaces d'apprendre.

WALKER ET ADAMS

MOTORISCHE FÄHIGKEITEN. DIE GÜLTIGKEIT MOTORISCHER SERIENTESTS ZUM VORAUSSAGEN DER FÄHIGKEIT ZUM MASCHINENSCHREIBEN

(Referat)

Ein Ueberblick der Fachliteratur zeigt, dass individuelle Unterschiede in feinen motorischen Leistungen im allgemeinen zu (1) einer allgemeinen motorischen Funktion, (2) zu einer Anzahl einfacher Faktoren, (3) zu einer Anzahl motorischer Grundfähigkeiten, oder (4) zu Unterschieden beim Ueben zuzuschreiben sind. Keine dieser Hypothesen scheint den Ergebnissen dieser und früheren Untersuchungen gerecht zu werden.

Siebzig Studenten (Anfänger im Maschinenschreiben) wurden mit sechs Tests der Stanford motorischen Fähigkeiten-Einheit (Koerth Verfolgungsdreher, motorischer Rhythmus, Serienunterscheidung, Klopfen, brauner Spulenpacker (Brown Spool Packer), und Geschwindigkeitsmotor geprüft. Nach siebenmonatiger Uebung im Maschinenschreiben wurden Mustertests für Maschinenschreiben gegeben.

Für die motorischen Tests rangierten sich die Gültigkeitswerte von ,74 bis ,93, während für die Schreibmaschinentests waren die Gültigkeitswerte ,89 für die Rohzahl der Stösse pro Minute, und ,6 für die Zahl der Wörter pro Minute nach der Strafe für Irrtümer. Wie bei anderen Untersuchungen mit der motorischen Fähigkeiten-Einheit (Motor Skills Unit) waren die Zwischenkorrelationen im Durchschnitt ,25. Die einzige Korrelation von irgendeiner statistischen Bedeutung zwischen den motorischen Testwerten und den Maschinenschreibenwerten war der Koeffizient von ,62 zwischen den Klopfenwerten und den Maschinenschreibenwerten (mit Berücksichtigung des relativen Gewichtes). Eine Nachprüfung bei sechsig Maschinenschreibenstudenten wies darauf hin, dass Klopfen nicht prognostisch für Maschinenschreibenfähigkeit ist, da die Korrelationskoeffizienten sich von —,1 bis ,176 erstreckten. Die frühere Korrelation von ,62 war wohl durch das starke Gewicht der Irrtümer veranlasst, wo die Gültigkeit von einem Test zum anderen nur ,28 war.

Die Eignungshypothesen werden als fraglich angesehen, wobei sich die Notwendigkeit von verschiedenen Methoden zeigt, wie wirksame Lernmethoden.

WALKER UND ADAMS

SHORT ARTICLES AND NOTES

ORGANIC PSYCHOLOGY: III. THE METHODS AND SUBJECT MATTER OF PSYCHOLOGY[1]

HULSEY CASON

PSYCHOLOGICAL METHODS

The methods of ordinary observation have been used for several centuries in collecting data on psychological topics, and the popular psychology which has resulted occupies a prominent place today in ordinary conversations, in the newspapers, and in moving pictures. This popular psychology includes much of what the people would like to believe, but its data are casual and haphazard. The psychology which we wish to discuss is scientific, experimental, and organic. It uses improved methods and obtains accurate, quantitative, and systematic data in many fields. Many organic processes are so complicated that even an experienced observer is unable to note everything that occurs, and in other cases the experimenter may not be able to remember long enough to make an accurate record. Many of the mechanical aids which are now used in psychology greatly increase the accuracy and reliability of the results that are obtained.

Observations on One's Self. An observer can generally make more accurate observations on other people and on animals than on himself, but there are some processes which he can observe best or can observe only in himself. The most common self-observations are concerned with traits of personality and with conscious behavior. Verbal activities are involved in making self-observations and in reporting to another person, and this introspective procedure is therefore referred to as the method of verbal report.

The most significant use that is made of verbal reports is in connection with the biographical method of studying personality, but high reliability and validity cannot be expected when the self-observations are concerned with such delicate matters as morals, intelligence, and feelings of inferiority. The method of verbal report is also frequently used as a check on other methods.

The Experimental Method. The essential feature of the experimental method is that accurate quantitative observations are made and reliable records are obtained on a few activities while various factors which might be disturbing, such as sounds, fatigue, practice, etc., are carefully

[1]Part I on "The Scientific Nature of Psychology" will appear in *The Journal of Philosophy* and Part II on "The Psychological Organism" will be published in the *Psychological Review.*

controlled. There are always several simultaneous activities in the psychological organism, and several different kinds of registering and recording apparatus may be practically indispensable. The principal purpose of the experimental method is to establish general principles.

The Test Method. In the psychological test the ability of a number of individuals with respect to a given function or group of functions is measured under approximately the same conditions, and the individuals may be rated or compared with each other on the basis of the data thus obtained. The processes involved in psychological tests are generally somewhat specific in nature, but when a large number of representative samples of a general function is measured the scores may have a general meaning. A disproportionate amount of time has been spent in attempting to improve the tests for general intelligence, but some valuable tests have also been devised for language aptitude, reading ability, sensory acuity, motor skill, learning, and personality traits. The aim of the psychological test has generally been the practical one of individual diagnosis and guidance, but the test results also constitute a notable contribution to the pure science of psychology.

The Genetic Method. The purpose of the genetic method is to determine the origin of all kinds of psychological processes, and there is no better means of understanding a process than by studying the conditions of its first appearance and the causal factors which account for its continued existence. Some attempts have been made to discover the extent to which almost every psychological activity is influenced by heredity, by environmental factors operating between conception and birth, and by learning and other factors after birth.

There is no point of view which has changed psychology as much as the genetic point of view. The study of sensory, nervous, muscular, affective, and intellectual functions in the young may be regarded as basic to an understanding of the same functions in the adult. In many investigations systematic observations are made on children of various ages and there is no attempt to alter the internal and external conditions of the subjects while the observations are being made. The most valuable form of the genetic method is the one in which this method is combined with the experimental and test procedures in attempting to establish general principles.

The Biographical Method. It is difficult to compress the life histories of different people into a single formula, and biographies have psychological value as well as human interest. The factors of heredity are different for all people except identical twins, and no two individuals have the same environments before or after birth. The need for the biographical method is increased by the radically different living conditions in different parts of

the world and by the very great differences between people in emotional experiences, knowledge, life ambitions, and adjustments and maladjustments to success and failure. In the urgent and often tragic task of studying a problem child or an abnormal patient, it is generally desirable to find out as much as possible about the whole life history, the events which led up to the trouble, and the present attitudes and symptoms. The combination of the introspective and biographical methods has given good results in the hands of some of the psychoanalysts.

The Subject-Matter and Fields of Psychology

Moment-to-Moment and Long-Term Psychology. One of the most general differences between the organic processes studied in psychology is in the duration of the processes. If an organic activity continues for only a few minutes and if it occurs quite infrequently with most people, it can generally be regarded as a relatively unimportant activity; but the general importance of such moment-to-moment activities is increased when the activities are frequently repeated. The inconsequential nature of many of the so-called experimental subjects studied in the "structural" tradition is better understood when considered in this way.

Long-term psychology includes all psychological conditions and activities which continue for a considerable period of time. Psychological heredity, for example, is of unusual interest because it has some influence on all kinds of conscious and non-conscious behavior throughout the whole life period; and general intelligence, the affectivities, habits, and desires are worthy of special consideration because of their long-term nature and influence. Moment-to-moment processes can sometimes be controlled and experimented upon more easily, but the long-term psychology is of greater general interest and value and there has been a definite shift of emphasis in the direction of long-term psychology in recent years.

The Different Fields of Psychology. The division of our science which is commonly referred to as "normal human adult psychology" does not receive the relative emphasis it formerly enjoyed because so many valuable studies have now been made on animals, children, and abnormals—and the words "normal" and "human" also have a relative meaning and a limited significance. "Physiological psychology" includes those subjects which are on or near the borderline between physiology and psychology, but physiological psychology should probably not be regarded as a separate division of psychology because all psychological activities are in some measure physiological in nature. "Experimental psychology" refers to the use of the experimental method—and if all of psychology is not experimental it is at least empirical on all occasions. Experimental psychology therefore cannot be regarded as a separate field. Some of the fields of

psychology can be marked off on the basis of the special interests of different psychologists, and on this basis the five fields which seem to be most clearly distinguished from each other are as follows.

1. Abnormal psychology. The oldest field of psychology has been concerned with the unusual and exceptional traits of human nature, and the most notable contribution of abnormal psychology is the observation that there is no sharp line of demarcation between normals and abnormals. Individuals are found who show all degrees of normality and abnormality from the most normal to the most abnormal. The surprising and interesting details of abnormal psychology are a valuable check on the generalizations of normal psychology. They show what can happen and what can be true.

2. Genetic psychology. Besides studying young people of various ages, this field includes a consideration of those facts in the technical subjects of genetics and embryology which have a bearing on psychological activities. Genetic data of prime importance have been obtained on the nervous system, glands, reflexes, native drives, learning, language, thinking, dreams, personality, and psychological deficiency. Most of the earlier studies which were inspired by the "child study movement" were unscientific, but the methods have been greatly improved in the scientific direction and the field is a very active one at the present time.

3. Differential psychology. Differential psychology is the study of individual differences between different people in all psychological traits and capacities, and so many investigations of the differences between people are made from time to time that psychology almost seems to be more concerned with differences than with similarities. The results of differential psychology have shown that individuals differ by degrees in every psychological trait and that people cannot be readily classified into types.

4. Animal psychology. Animal psychology is the study of the psychological activities of subhuman animals, and there is sometimes an advantage in using subjects who have no attitudes or convictions on the technical problems of academic psychology. It may be possible to control the bodily processes of an animal and some of the conditions surrounding him better than in the case of human subjects, and operations can be performed on animals which would be somewhat embarrassing with people. Special strains of animals are sometimes used for experimental purposes, and in certain types of investigations such as those concerning the relative influence of native drives and learning the genetic history of the animals should be known in some detail.

Some of the animal and comparative psychologists are principally interested in the animals themselves, and they are not particularly concerned over the extent to which the data they obtain with animals can be applied

to man. The general aims of these investigators are similar to those of animal zoologists, but a considerable portion of animal psychology has some application to human nature. The extent to which the results obtained with an animal will carry over to man depends upon the extent to which man and the animal in question resemble each other in anatomy and physiology, and many of the gross psychological differences between men and animals have been correlated with gross differences in bodily structure and function.

5. *Applied psychology.* Investigations in pure science are generally pursued without any special concern over their immediate money value, and the most valuable contributions to psychology have been made by investigators who did not have any special interest in applications. All subjects which attain the status of a science include a certain amount of material which has some practical applications, but the most valuable principles as far as the science itself is concerned are frequently just those principles which have the greatest transfer value to other scientific and intellectual fields. It is fair to say, however, that several psychological studies which have been prompted by a purely practical motive have been entirely scientific and have yielded worthwhile data for psychology.

The first question which should be asked about applied psychology is the question "Applied to what?", because one is frequently by no means certain to what things the applications are to be made. The most practical benefits to be derived from the study of psychology are (1) an increased knowledge of the psychological activities themselves, (2) some clarification of many questions relating to such subjects as motivation, interests, and attitudes which would ordinarily remain obscure, and (3) an increase in the ability to predict and control a few special types of human behavior. A common and sufficient reason given by psychologists for carrying out experimental investigations is that it amuses them; and many of the most exciting subjects in psychology are generally not thought of as being applied at all, unless it be applied to the not unimportant occupation of living.

The Aims of Experimental Psychology

In every science an attempt is made to bring together the facts and principles which have as wide an application as possible, and experimental psychology is the central body of psychological knowledge to which all of the special fields are related. This central body of knowledge is scientific, experimental, and organic. It includes activities that are found in the normal and in the abnormal, in adults and in children, and in men and animals. All psychological methods have contributed their share of information to this central field.

Although the principal portion of psychology is now experimental and scientific, the psychological calm has been disturbed at various times in the past by the expression of radical views, especially on subject matter and method. The civil warfare has been carried on with heavy artillery on some occasions and an exaggerated impression has gone abroad in regard to the amount of controversy which actually exists in our subject. The psychological front is fairly calm at the present time and explosions and eruptions are not as common as in the years just past. The best established facts about the psychological organism arrange themselves in a fairly natural manner and with only a small amount of straining. One often hears the statement that all psychological methods are of value and that each field is a worthy division of the science as a whole.

The large majority of psychologists believe that each psychological method and field has an unquestionable value; but this does not mean that the majority has adopted an "eclectic" point of view. The word "eclectic" has a philosophical and not a scientific reference. It means that a philosopher has attempted to construct a philosophical system, has become confused, and has failed. The scientific advances in psychology have been progressive and irrevocable, and several features of the psychological organism have been investigated by a kind of interdepartmental research. All of the functions studied in psychology are organic functions and the older interest in constructing a philosophical system out of or in psychology has practically disappeared. Experimental sciences do not have a philosophical and therefore cannot have an "electic" point of view, and a present-day psychologist in making a fairly free use of all his methods and the data from all of his fields is no more "eclectic" than an anatomist who studies the heart as well as the head or a physiologist who studies digestion as well as respiration. Psychology studies and has to study many different kinds of organic processes because of the many-sided nature of the psychological organism.

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CONTRAST INDUCED BY COLOR SO FAR REMOVED INTO THE PERIPHERAL FIELD AS TO BE BELOW THE THRESHOLD OF SENSATION

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In previous papers (5, 6) three situations are described in which contrast sensations may be aroused by colors which are not themselves sensed. It is the purpose of the present paper to describe another set of conditions under which contrast may be sensed when the exciting stimulus is below the threshold of sensation.

The original experiments were conducted before we had made our determinations which showed that red, blue, and yellow spectrum lights when of sufficient intensity can be sensed as far out as the limits of achromatic sensitivity (4). We had been compiling evidence which seemed to indicate that more than one functional level was needed to explain the phenomena of color vision. It was our conception that one of these levels, which might be called the receptor level, was the seat of the selective reaction to wave-length, the inhibitive or cancelling reaction of the complementary colors, and the after-image and contrast reactions; but the assumption of other levels was needed to explain the elaboration of the initial reaction into sensation, and such phenomena as the effect of the achromatic on the chromatic excitation and the ability to get after-image and contrast sensations in certain color-blind patches which occur in the peripheral retina of the normal eye. With reference to the former phenomena it will be

remembered from an earlier paper (1) that, when suitable precautions are observed, both the hue and the saturation of a color can be changed by the action of an achromatic excitation without detectably affecting either the hue or saturation of the after-image or contrast sensations aroused by the color. This seemed to indicate that, at least under conditions prescribed, the action of the achromatic on the chromatic excitation must take place at some level posterior to the after-image, contrast and cancelling reactions. With reference to the latter phenomenon, it will be remembered also that results were given in a former paper (2) to show that a careful examination of the peripheral field discloses small areas or patches of insensitivity to one color in which the after-image and contrast reactions to that color and its inhibitive or cancelling reactions on the complementary color do not appear to be affected. This seemed to indicate that the deficiency which prevented the color from being sensed was posterior to the level of the after-image, contrast, and cancelling reactions. These areas were called by us Schumann spots because Schumann (7) had described a case in which the eye was blind to one color but had the after-image, contrast, and cancelling reactions proper to that color. So far as we know, however, the existence of patches in the normal peripheral retina of the kind described above was unknown to Schumann; it was, therefore, somewhat misleading to apply to them the name, "Schumann spots."

There was in addition the somewhat remarkable fact that the proportions in which the eye balances its complementaries to white remain a constant from center to periphery of the retina despite the great range of variation in the ratios of sensitivity to those colors in the peripheral field (3). It did not seem wholly improbable, therefore, that functions which we have considered to belong to the receptor level, namely the cancelling, after-image, and contrast reactions, might all be present in eyes and in parts of the same eye in which the color sensation itself could not be experienced and that this might be the situation in the color-blind portions of the peripheral field. If that were the case it might then be possible to arouse the contrast excitation in these portions of the retina and to detect it by allowing it to spread centralwards into those parts of the retina in which color could be sensed. Such considerations as these were the primary incentive which led to the experiments to be described in this paper.

It was found that, when the peripheral field was filled in with color so far removed from the center that it was below the threshold of sensation and the more central portions of the field with gray of the brightness of the color, contrast could be induced over this entire gray area by using the special method of sensitizing contrast described in former papers (5, 6). While the results obtained were in accordance with the hypotheses noted

above, doubt was cast on the need for the assumptions made in this hypothesis by the discovery later that the far periphery of the retina is not blind to color. However, the gradient of sensitivity from the mid to the far periphery is very steep. Two possibilities then remain: (1) The impairment of function which causes the low sensitivity is posterior to the level of the selective response to wave-length, the cancelling, after-image, and contrast reactions; in which case the above reactions may not be impaired at all or at least not in proportion to the loss of sensation. And (2) that all these functions are impaired in proportion to the loss of power to sense color but that the induced color was raised above the threshold by the application of a specifically sensitizing technique.

While this might account for the results obtained in our experiments in contrast, it does not account for the fact, as brought out in later experiments, that while the ratio of sensitivity to the complementary color varies enormously from the center to the far periphery of the retina, the proportions in which they balance to neutrality are not altered. The latter point can be more safely stated perhaps by saying that the proportions required to balance to neutrality at the center are not the same as the ratios of sensitivity at the center and are widely different from these ratios towards the periphery, but that a balance made at the center holds throughout the periphery. How much these proportions could be deviated from at various points in the periphery and still give neutrality, we are not prepared at this time to say. That is, the degree of imbalance towards one or the other of the excitations might be great and still not be enough to be sensed as color in the more remote parts of the field, in which event there would be of course a corresponding change in brightness towards that of the dominant component. All things considered, however, from definite knowledge of the conditions at the center and what we know of the conditions in the periphery, it seems safe to say that the power of a color to cancel or balance its complementary color is not the same as its power to arouse sensation. There seems thus in any event to be a separation of this balancing function, whether it be synthetic or inhibitive, from the power to arouse sensation. However, that can be very simply stated as a fact without entanglement with theoretical dogma.

The experiments were conducted in a room 15 feet long, 8 feet wide, and 10 feet high. The walls and ceiling of this room were of rough unpainted plaster. At one end of the room was a window 2 feet wide and 3 feet high. The bottom of this window was about 3 feet from the floor. The window was provided with a heavy light-proofed door or shutter. The room was in general light-tight. In front of this window and receiving its full illumination was a tangent screen mounted on a table. In front of the center of this screen at a distance of 75 cm. was mounted a mouthbit

on a secure support. The mouthbit was covered with wax in which the impression of the observer's teeth had been made and hardened. The screen was covered with gray paper of the brightness of the color to be used. At the center of the screen was a fixation cross. The field for the color was first mapped in 16 meridians with a stimulus subtending an angle of 1 degree of arc at the eye. Beyond the field as mapped the screen was filled in with colored paper. This paper, because of the massiveness of the stimulus, could always be sensed. It was necessary, therefore, to widen the field by cutting away the borders of the paper until the color could no longer be seen. When this was done the observer took the fixation at full illumination. At this illumination no contrast could be sensed over the gray field. However, when the window was closed, the contrast color appeared over the entire gray field while the illumination was changing. The effect was the most pronounced when the inducing color was red. It could be obtained, however, with other colors of good saturation. The Hering standard pigment colors and grays were used. Again, as in the former papers in this series, it is well to state that important conditions for the success of the experiment are daylight illumination, a sufficient range of change of illumination, and a quick uniform rate of change. Artificial light is especially unfavorable to the success of the experiment, and a favorable rate of change of illumination is very difficult to secure with curtains.

An interesting variation of the experiment was to hold the fixation until time would have been given for the eye to become fairly well adapted to the color could it have been sensed, e.g., 40-60 seconds. Then, when the illumination was decreased, at a point in the decrease the gray appeared in the hue complementary to the contrast color, i.e., in the hue of the inducing color. In explanation of this we may recall a well-known phenomenon. That is, when one looks at a colored surface from 40-60 seconds and the illumination is quickly decreased the surface observed appears strongly in the complementary or after-image hue. In this case the marginal color could not be sensed, but at a point in the decrease which might be assumed to be the point at which the marginal color shifted its hue to the complementary color, the contrast induced also shifted to the complementary hue. That is, when the marginal color was red, the contrast induced was green until the critical point in the decreasing illumination was reached, at which time it changed to a wash of unsaturated red. The contrast colors, as might be expected, were faint in this experiment because the border of marginal color was comparatively narrow and the area over which the induction took place was great.

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MAZE-CONSTRUCTION AND THE ROLLING-BALL MAZE

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The maze technique has been much used and somewhat abused in psychological experiments. Originally it was the landscape gardener's method of amusing people by giving them the experience of being "lost"; then it was utilized by psychologists to keep animals in torture; finally human beings are themselves subjected by thousands to this same kind of annoyance with but little amusement. As Esper has recently remarked (13; cf. also Miles, 33), "the history of mazes in psychology is somewhat amazing." To the present writer this history illustrates the fact that the psychologist's own research behavior is trial and error in nature and lacks insightful deliberation, and that the experimenter himself, confounded by blinds and alleys in the maze of techniques, can only follow the line of least resistance. It is not the purpose of this short note to criticize the maze technique in general but to point out the distinction between maze-pattern and maze-construction, to summarize some of the main varieties of maze-construction, and to describe the rolling-ball maze.

Maze-Pattern vs. Maze-Construction

The concept of "maze" is varied, indefinite, and very loose indeed. The term has, in current usage, at least two distinct meanings. Of course, when we speak of the Hampton Court maze of the early days we can mean only the pattern with the manner of construction understood. Similarly Stone's multiple-T maze or Warden's T maze refers only to the pattern. But in such designations as Miles's finger maze, Hunter's temporal maze, or Esper's latest polytactic maze, the meaning of the term changes from that of a definite *pattern* of routes to be followed, traced, and pursued, to the nature, manner, or material of *construction* of the maze-pattern. The writer (9) pointed out this significant distinction in connection with one variety of universal maze-construction apparatus paradoxically named "the pattern box." But much confusion still exists in the literature concerning this indiscriminate use of the single word "maze," sometimes in the sense of pattern and at other times in the sense of construction. An exhaustive collection and analysis of all existing maze patterns will perhaps reveal some of the fundamental problems of the maze technique. But in this short note the writer is concerned only with maze-construction.

Varieties of Maze-Construction

There are several notable changes in the evolution of maze-construction. Before 1927 all maze constructions were of the alley type, except perhaps McDougall's introduction, at the suggestion of Li Chi, of the water maze, used in his series of experiments on the Lamarckian theory. Some of the subsequent significant modifications of maze-constructions are Hunter's temporal and tridimensional arrangements (1928, 1929), Warden's standard unit (1929), and Walton's reversal to the vertical (1930), culminating in Esper's polytactic manual-movement apparatus (1933). A brief summary may be given under the following headings.

Permanent Record. The first conscious improvement in the maze technique is the facility for permanent record made possible by Perrin's method first used in 1912 (39), later adopted and perfected by Boring (5), Franz and Watson (16), all of whom made use of paper recording of the tracings of a stylus pencil. This is the first step toward elaborate methods of self-recording. Automatic devices had been provided in 1925 for animal maze-running by Tolman and Jeffress (47), later improved and modified by Tolman, Tryon, and Jeffress (48), and also by Buel (6), and in 1928 by Miles (32). While Tolman's control is electrically operated and includes an automatic delivery table, Miles's extremely simple method is to use blotting paper for recording a maze graph of the foot-steps of the rat, whose relative running-time in different parts of the maze and the absolute duration of stay in any section of the maze may be directly

retained. In the Michigan demonstration maze board described by Ford (14), we may have a self-recording maze for humans. Slater (45) described a self-recording human maze which records objectively and permanently all errors and which can be used with any kind of maze pattern that can be drawn on coordinate graph paper.

Showalter (44) recently described a variable pattern maze for humans, which is essentially a checker-board alley maze with electric recording apparatus. The recording apparatus consists of a set of five coils connected to ground of a transformer and a phonograph motor arranged to pull a piece of adding-machine tape between five pairs of points. The maze technique had reached perhaps its climax of instrumental control when Esper (13) reported his polytactic maze. Among other notable characteristics, the elaborate electrical recording control is certainly one of the very few attempts at rigid instrumental perfection in order to avoid as much as possible all sorts of variable errors in psychological experimentation.

Variability of Pattern. The second notable advancement in maze-construction is the possibility for variable and reversible patterns. One of Perrin's (40) early forms of stylus maze is a slotted brass plate and hence can be used on the reverse side. He was also the first to use steel strips or plugs to be inserted on a parallel groove checker board to permit variable patterns to be set in at will. This method was later used by Pechstein (37) and Koch (22), while Miles (21) nailed uniform pieces of wooden blocks on tin and board so that the pattern can also be varied, although to a less degree.

But Chou's (9) universal finger maze, the pattern box, was the first deliberate attempt to produce an elaborate instrument that permits maximum amount of variability in pattern. Barker's (2) stepping-stone maze is also designed for universal pattern construction. This characteristic is present in Showalter's (43) paper maze consisting of a series of numbers arranged in haphazard order. Both Showalter (44) and Esper (13) made variability in pattern one of the leading requirements in their most elaborate electric self-recording mazes. Stetson and Dashiell (46) were the first to devise a multiple-unit system of maze-construction, which was followed by Warden's standard-unit maze (52) and by Liggett's sectional maze (23). Miles's elevated maze for rats is fundamentally universal and variable in pattern (30).

Temporal Alternation. The third significant departure in the maze technique is Hunter's introduction of the double-alternation temporal maze (19). So far all maze-constructions are limited to spatial orientation on a plane. It is evident that the same spatial units may be traversed by variable temporal alternations not unlike the spatial alternation of left

and right. The temporal maze as used by Hunter is for rats. But Barker (1) has recently also adapted it for humans by finger tracing in his temporal finger maze.

Visual Control. The fourth significant contribution toward maze-construction is Carr's (7) slot maze in which the blind alleys are hidden from view so that the entire maze pattern may be used without blindfolding. Modified and improved forms of this type of visual control have been furnished by Bodkin (see 26), McGinnis (26), and Griffith (17). In Perkins' maze of fixed orientation we have the visual field of the maze pattern controlled by looking through a tube (38). The subject traces the maze only a very limited portion at a time. Here the orientation is fixed. In Miles's two-story duplicate maze (29), however, the orientation is somewhat distorted, not unlike that in mirror-drawing. Such variable pattern constructions as Barker's stepping-stone maze, Showalter's paper maze, and Esper's polytactic maze are also invisible in nature. Cook's star-shaped maze (10) is of the Carr slot type.

High-Relief and Elevated. Perhaps the most significant change in maze-construction is Miles's high-relief finger maze for humans (31) and his narrow-path elevated maze for rats (30). It was Meyer who first suggested the use of the finger instead of a stylus to trace the half-round grooves in iron casting (27). This method has been recently restored by Barker in his temporal finger maze (1). Chou suggested numerous ways of producing the discrete-path, high-relief finger maze by the use of shots, ball-bearings, pins, nails, and staples (9). Corey's inexpensive finger maze makes use of staples (11) previously employed by Chou.

Vincent's early innovation of taking away the walls of alleys (50) was the forerunner of the elevated type of maze construction. Dennis (12) has recently modified Miles's method of the frames by using a series of one-piece wooden blocks standing on their sides.

Other Characteristics. Numerous other modifications or improvements in the construction of mazes have also been suggested by investigators. Heron (18) has equipped the end-path with electrical contact so as to give a goal signal; McDougall (25) has adopted Li Chi's suggestion of a water maze, where the fundamental idea of maze-running is changed to maze-swimming and -diving in or under water. Schlosberg and Carmichael (41) have cast in cement, by the use of a wooden mold, a stylus maze of triangular cross-section alley or V-shaped groove to be run with a ball-pointed stylus. Back tracing is prevented in Scott's improved stylus maze of inclined groove (42) and in Esper's polytactic maze. Trueblood's so-called tunnel maze (49) with glass floor and top mounted on a swivel, permitting rotation to any position, is in reality not much different from the ordinary alley tunnel with wire mesh as top. Bills (see Foster and Tinker, 15,

p. 379) seems to have been the first to describe a vertical animal maze, which was later used by Walton (51) for lecture-room demonstration. Hunter's tridimensional maze for animals uses Miles's narrow-path method to form a series of spiral paths to be climbed by the rat (20). Miles's so-called unidirectional and midway mazes (34, 35, 36) are however, varieties of maze-pattern rather than of maze-construction.

Summary of Methods of Maze-Construction. The above discussion may be briefly summarized as follows:

An Outline of Methods of Maze-Construction

I. Permanent Record
   1. Pencil tracing (Perrin, 1912; Boring, 1913)
   2. Maze graph (Miles, 1928; Slater, 1932)
   3. Electrical self-recording (Tolman and Jeffress, 1925; Ford, 1931; Showalter, Esper, 1933)

II. Variability of Pattern
   1. 1914—Perrin through-slot reversible
      —Perrin inserted strip or plug
   2. 1919—Stetson and Dashiell multiple unit
   3. 1927—Miles elevated hurdle frames
   4. 1928—Miles nailed strip
   5. 1929—Chou's inserted pins, clips, or staples on perforated tin
      —Warden standard unit
      —Liggett sectional
   6. 1931—Barker electrical stepping stone
   7. 1932—Showalter numerical paper
   8. 1933—Showalter electrical checker board
      —Esper electrical polytactic

III. Temporal Alternation
   1. Hunter's double-alternation temporal maze (1920)
   2. Barker's temporal finger maze (1931)

IV. Visual Control
   1. 1921—Carr slot (Bodkin, 1927; McGinnis, 1928; Griffith, 1931)
   2. 1927—Perkins tracing tube (fixed orientation)
      —Miles two-story duplicate
   3. 1931—Barker stepping stone
   4. 1932—Showalter numerical paper
   5. 1933—Esper electrical polytactic
      —Cook star-shaped

V. High-Relief and Elevated
   1. Finger (groove and high-relief)
      a. Meyer groove (1922)
      b. Miles high-relief (1927)
      c. Chou's perforated tin (1927)
      d. Corey inexpensive staple (1931)
      e. Barker temporal (1931)
   2. Elevated (frame and block)
      a. Vincent wall-less flat bottom (1912)
      b. Miles narrow-path (1927)

c. Hunter tridimensional (1929)
d. Dennis block (1931)

VI. Other Characteristics
1. Heron electrical goal signal (1924)
2. McDougall-Li water-swimming and -diving (1927)
3. Bills vertical (1927)
4. Scott inclined groove (1929)
5. Trueblood tunnel (1929)
6. Hunter tridimensional (1929)
7. Miles unidirectional and midway (1931)
8. Schlosberg and Carmichael V-shaped groove (1931)
9. Esper polytactic (back tracing prevented as in Scott inclined groove, 1933)

The above more or less complete outline of all types of maze-construction may suggest several criteria for a classification of this important, much-used, and highly variable instrument in psychological experimentation. Maze-constructions may be either *self-recording* or *non-recording, variable* or *fixed, temporal* or *spatial, visible* or *blindfolded, water* or *dry, vertical* or *horizontal, groove* or *elevated, abstract* or *concrete, diagrammatical* or *material,* and perhaps amenable to permanent graphic recording of both time and error scores, as in the case of Esper's recent perfection, or still left to all sorts of constant and variable errors of experimentation. These are some of the characteristics judged from the point of view of the maze as an instrument. But investigators are likely to be blinded by the multitude of possibilities of construction and to lose sight of the nature of the very maze performance itself. It is evident that, as the type of material used for constructing a maze changes, the nature of the maze performance changes with it. The writer is of the opinion that concentrated, exhaustive, and intensive study on one form of maze performance in connection with one particular type of material make-up will certainly contribute more toward the psychology of maze learning than any number of researches or notes on the varieties of pattern and construction possibilities.

An Analysis of the Maze-Performance

Judging from the notorious varieties of maze-patterns and maze-constructions, we are startled to find that the *maze performance* is extremely unstandardized. It is the most frequently employed technique in the whole field of psychological experimentation. From the study of pure manual skill to that of the highest form of mental functions, the maze technique has been constantly taken as the most available form of attack. Just what is this common essence in the maze performance that so much appeals to psychologists? Is there a common factor in this supposedly identical performance, which at first glance seems to be as different in different situations as there are number of different types of pattern-construction

make-ups? Are we abusing the concept of "maze" when we mean by it both the most abstract reasoning process and also the most simple eye-hand coordinations? The whole psychology of the maze needs to be entirely revaluated, fundamentally weighed, and perhaps once for all it uses and limitations clearly denoted. Space does not allow here for a detailed discussion, although the writer wishes first to point out some features of the indefinite nature of the maze performance.

Walking, Running, and Swimming. Traditional maze-performances can be grouped into three types, namely, *walking* (including running and swimming), *tracing* with stylus, finger, or eye-hand, and *associating* implicitly. The original Hampton Court maze is of life-size for humans to walk in. When it was adopted for the use of animals, the running activity demanded is still like that of the human walking; but when the body of the maze pattern is totally submerged under water, as McDougall did, the performance is no longer walking or running but swimming and diving. However, no matter whether the subject is walking, running, or swimming, the whole organism is responding to the total situation, with no unnatural alteration of the subject's sensory make-up. It is not unlike the situation where one is totally lost in such a place as the railway station, the crowded street, or the big department store. The nature of this performance is distinct from either of the other two types, because it involves the total responding organism.

Tracing with Stylus, Finger, or Eye-Hand. But when the maze pattern is reduced to miniature form and the performance changed to purely manual tracing with stylus or finger and visual-manual pursuing with eye and hand, the nature of the stimulus-response pattern is entirely different. Total response is broken into part responses of certain limited groups of musculature, where the responding organism is saved from the extremely intense feeling of utter loss, helplessness, and despondency. Implicit subvocal aids are of more importance here than in the walking, running, or swimming performance, because there is no immediate necessity for the subject to respond quickly and decidedly as in the case of the water maze-swimming, for example, where the organism cannot afford to wait. But this second type of maze performance is still not so free of peripheral part-activities as the third type.

Associating Implicitly. Implicit associating in the so-called "mental maze" is totally different from either of the first two types of maze-performance. Further narrowing down of the response repertoire to only the verbal-motor makes the very performance so complex that it resembles more the higher mental processes than the original total-organism response. Whether it is legitimate to include this type of implicit performance of the problem-solving variety in the already over-inclusive category of "maze"

is another question that the writer does not wish here to discuss. The above contrast between the running, tracing, and associating differences in the maze performance is not so much a significant distinction as to warrant universal recognition; but the point is that it serves to bring out into high relief the fourth type of maze-performance recently introduced in the rolling-ball maze.

Ball-Rolling vs. Stylus-Tracing Mazes

The writer described in 1929 a technique for testing motor skill, which is in reality a rolling-ball maze (8). The stimulus-response pattern involved in this new and extremely simple method is essentially the control of the action of a rolling steel ball along definitely prescribed grooves on a multiple-groove board by tilting the entire board with two hands.

Boder (4) reported in 1930 "a tridimensional maze," which also makes use of a steel ball-bearing to be rolled around freely inside a series of brass pipes—ordinary gas fixtures—which are screwed and soldered together in various directions. Openings are left on the ends of the pipes, through which the ball may enter or fall. Finally Mattson (24), in a monograph on child learning, reported in 1933 "a rolling-ball maze" that is essentially the idea of the writer's earlier rolling-board technique. A few words of elaboration on the distinction between the *stylus-tracing* and the *ball-rolling maze techniques* seem to be desirable.

Origin of the Rolling-Ball Performance

The writer had long been interested in that type of puzzle in which as many ball-bearings or shot are present inside the box as there are holes on the horse-shoe or other design. The task of the game consists in moving the box in such a way that each ball will rest finally in one of the holes on the design. It requires some skill and much patience.

This rolling-ball game was suggested to the writer when he designed the *discrete-path universal finger maze* in 1929, to use shot, nails, balls, pins, and staples to construct the maze board. But from stationary balls arranged in a line to form the high-relief finger maze to a single ball rolling to be guided along prescribed grooves is, I think, a very significant advance in technique. The writer had previously worked for some time in producing a variable pattern finger maze out of 3/16-inch ball-bearings, for which several designs had been suggested (9).

Characteristics of the Rolling-Ball Maze. Instead of watching a rat run or guiding a stylus along the grooves or alleys of a maze-pattern, the experimenter or subject adjusts a groove board in such a way that a freely rolling ball may run along a series of paths at his command. In this case, we are measuring not the intelligence or stupidity of the rolling

ball but the skillfulness or clumsiness of the person who is guiding the ball, because all along the grooves there are many treacherous by-paths and the ball has every chance of rolling into the wrong path, and it is only through constant alertness in eye-hand coordination in adjusting the entire board to compensate the inertia or momentum of the ball that a normal person can satisfy, for example, the criterion of three perfect trials in succession.

Here the reaction called for is neither that of aiming nor of tracing. It is genuine "pursuit" in the sense that the tilting of the entire board should be in perfect unison with the ball's movement along the true path and this unison should be maintained throughout the entire act. It most resembles the adjusting movements in the Miles pursuitmeter (28), except that the whole trunk and the legs and feet are involved in the adjustment. A better situation, a simpler technique, a stimulus-response pattern more closely related to actual conditions of human performance, could scarcely be found.

Certain features are characteristic of the rolling-ball maze in contrast to the tracing-stylus maze. The performance is indirect; the coordination is more complex; and the sensorimotor control is perhaps much more difficult. On the other hand it is more lively, natural, and challenging. As Mattson (24) has pointed out, it fulfills at least six requirements:

1. A skill which lends itself to a trial-and-error method of attack rather than sudden complete solution.
2. A skill which has within it different degrees of difficulty, or complexity.
3. A skill which is novel so that the subjects will be relatively unpracticed at the beginning of the study and will have no opportunity for additional practice between periods during the experiment.
4. A skill which is suited especially to the age and the ability of preschool children, being neither too easy nor too difficult for the particular group of subjects but capable of differentiating between them.
5. A skill in which the learning is very rapid so that one can get measurable improvement in a relatively short time.
6. A skill that is interesting and appealing to young children.

Summary and Conclusions

The term "maze" denotes at least three distinct meanings, namely, *pattern, construction,* and *performance.* In order to avoid needless confusion in terminology, the writer suggests that the intended meaning of the term should be designated by using the corresponding compound expressions, namely, *maze-pattern, maze-construction,* and *maze-performance.* At least the term "maze" should not be used indiscriminately for both pattern and

construction at the same time, as in the expressions of "Stone's multiple-T maze" and "Miles's elevated maze." If the compound expressions are somewhat cumbersome and objectionable to use every time we refer to a pattern or construction, it is suggested that at least the "pattern" meaning should be explicitly expressed. We may write, instead of Warden's T maze, simply "Warden's T-pattern." If this suggestion is followed, then the term "maze" will be hereafter limited more or less exclusively to the type of construction or to the pattern-construction combination in general.

Numerous varieties of maze-construction have been suggested by various investigators, which lend themselves to (1) permanent record, (2) variability of pattern, (3) temporal alternation, (4) visual control, (5) high-relief and elevated, and (6) other characteristics, such as goal signal, water submersion, V-shaped groove, back-tracing prevention, rotary orientation, vertical arrangement, tridimensional arrangement, and what not. Several criteria may be used to classify maze constructions. The general make-up of the maze may be either self-recording or non-recording, variable or fixed, temporal or spatial, visible or blindfolded, water or dry, vertical or horizontal, groove or elevated, abstract or concrete, diagrammatical or material, and so forth.

An analysis of the maze performance reveals four types of stimulus-response pattern, namely, (1) walking, running, or swimming; (2) tracing with stylus, finger, or eye-hand; and (3) associating implicitly, besides (4) ball-rolling, which is characteristically different from the first three types.

The rolling-ball maze was first described by the writer in 1929 in the *Journal of Experimental Psychology*. Boder employed the ball in 1930 in his tridimensional maze, while Mattson in 1933 used exactly the writer's technique almost without alteration in principle. Since this ball-rolling performance is significantly different from any of the former maze performances, both its origin and its chief characteristics have been given in this article. It is noted that, besides being a lively, natural, and challenging activity, this new maze performance is trial-and-error in nature, of different degrees of difficulty and complexity; it is novel, suited to preschool children, easily learnable, and interesting and appealing.

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AN APPLICATION OF THURSTONE'S METHOD OF FACTOR ANALYSIS TO PRACTICE SERIES

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Thurstone's simplified multiple factor method of analysis was applied to a practice series in an attempt to find a factor pattern which would describe the scores made in successive practice periods in a single task.[1] This analysis was repeated for four different tasks.

The subjects used were all of the grade 4B pupils in P. S. 165 in New York City. There were 99 in all at the start. However, because of absences, our final results are on a group of about 60 subjects. Since the differences between the scores of the boys and the girls were not statistically reliable, we felt justified in including both boys and girls in the group.

The tasks used were:

1. Making Gates. The subjects were given lined paper and required to make rows of little gates, each gate consisting of 4 vertical strokes and 1 horizontal one. They were instructed to make as many as they possibly could in the time allotted, and were allowed 1 minute for each trial. The score was the number of gates completed.

2. Whipple's Symbol-Digit Test. Two minutes were allowed for this test. On one day the subjects would begin work on the first column, and on the next day begin with the second column, alternating back and forth so as to minimize a specific memory element from day to day. The score was the number correct.

3. A vocabulary form of substitution test known as the Turkish-English Vocabulary Test was used. The subjects were each given a key sheet containing 20 Turkish words with their English equivalents. They were also given a test sheet containing only Turkish words. These long lists

[1] A report of this work was read at the April, 1934 meeting of the New York Branch of the American Psychological Association. The writer is indebted to Professor H. E. Garrett of Columbia University for suggesting the problem.

of Turkish words were composed of the 20 on the key sheet selected and repeated in random order. The task was to write next to each Turkish word on the test sheet its English translation looking back at the key sheet whenever necessary. The subjects started on different columns on the test sheet on different days. Four minutes were allotted for each trial. The scores consisted in the number of correct translations made.

4. This task was an arithmetic test of an unusual type. (Suggested by the Dearborn Group Test of Intelligence.) The subjects were given mimeographed sheets of paper containing addition examples and also the answers. However, one number in each column of the addition was omitted. The subjects had to enter the correct number so as to give the answer as printed. Several equivalent forms of this test were employed. Four minutes were allowed for each trial and every correct entry counted one.[2]

Twenty trials were given on each test, one trial per day on consecutive days excepting Saturdays and Sundays. The first trial was considered as preliminary practice equivalent to more precise directions, and therefore has been omitted from the records. The remaining 19 periods for each test furnished the data used.

In the analysis each task was dealt with separately and independently of the other tasks used. For simplicity's sake, 5 trials were selected as representing the practice series as a whole. We chose the 1st, 3rd, 6th, 12th, and 19th trials for this purpose.

The intercorrelations of scores on these different trials of the same task were computed, and we found that for each test they tended to be arranged in a hierarchy; i.e., the 1st trial correlated higher with the 3rd than it did with the 6th trial, it correlated higher with the 6th than with the 12th, and so on. The nearer together in the series trials fall, the higher the intercorrelations of scores on these trials.

When applying Thurstone's center of gravity method of analysis[3] to these correlations, we derived factor loadings as shown in Table 1. In each case the first factor loadings seem to increase from the first trial to about the middle trial used, and then decrease again to the end of the series. The second factor loadings seem to form a hierarchy going from a fairly large plus on the first trial to a low plus, then to a low minus, and then to a larger minus on the last trials in three tests, and do the same thing with signs reversed in the case of the Symbol-Digit Test. With the number of trials used we can correctly derive only two factors

[2]For a more complete description of subjects, tests, reliabilities, etc., see Perl (1).

[3]For a description of the method see Thurstone (2).

TABLE 1
FACTOR LOADINGS

Trial	Making gates I	II	h^2	Trial	Turkish-English I	II	h^2
1	.727	.393	.683	1	.843	.385	.859
3	.848	.214	.765	3	.877	.308	.864
6	.861	—.112	.754	6	.932	—.085	.875
12	.795	—.355	.758	12	.920	—.268	.919
19	.778	—.190	.641	19	.753	—.354	.692
K^2	3.226	.375	3.601				
$\frac{K^2}{N}$	.645	.075		$\frac{K^2}{N}$	.752	.089	

Trial	Arithmetic I	II	h^2	Trial	Symbol-Digit I	II	h^2
1	.867	.238	.809	1	.725	—.340	.641
3	.913	.040	.836	3	.939	—.212	.926
6	.930	—.190	.901	6	.900	—.193	.847
12	.932	—.188	.904	12	.830	.285	.770
19	.915	.046	.838	19	.645	.421	.594
K^2	4.156	.132	4.288	K^2	3.322	.456	3.778
$\frac{K^2}{N}$	.831	.026		$\frac{K^2}{N}$	.664	.091	

$\frac{K^2}{N}$ is the percentage of the variance attributable to the factor in the column in which it stands.

h^2 is the percentage of the variance of a trial explained by two factors here derived.

as one ought not to derive more factor loadings than one had intercorrelations to start with.

Now we have the dangerous but necessary task of trying to say what these factors mean, dangerous particularly because these factors are statistical entities but not necessarily psychological ones, and necessary because an analysis such as this can have very little meaning for psychology unless the factors which have been found to describe a table of intercorrelations are interpreted.

On first thought one might expect that the first factor would represent whatever ability it is that the test was testing. But since that is supposedly a constant it would not show up as a factor determining the different scores on successive trials. The first factor seems rather to be what might be called a "proximity" factor. We noted that scores on a trial usually correlated higher with scores on nearer trials than with scores

on trials further away in the series. The middle trial of a series would always be nearer the other trials and therefore have a higher average correlation with the others than any one other trial. Therefore, if this first factor is something related to proximity, we should expect it to have the greatest weight in the middle trial and have less weight in the trials that came earlier or later. Looking at the distribution of the first factor loadings, we can see that for each test something like this does seem to occur. We would tentatively suggest that in these practice series the first factor might be something like "proximity."

The second factor loadings, forming a hierarchy from plus to minus (in one case from minus to plus) as they tend to, seem to indicate that the second factor is related to practice. It might be associated with the process of becoming emotionally adjusted to the task, or the presence of distracting influences or susceptibility to them, or almost anything related to the learning process itself. The learning that takes place in a practice series seems to show up in the second factor loadings.

It is interesting that this analysis, applied to scores of trials in a practice series of a task rather than to test in a battery, gives fairly similar results in the case of four such different tasks.

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APPARATUS

SYNCHRONOUS TIMING DEVICE*

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Every laboratory has almost daily need of an accurate timing device. The seconds pendulum is more or less standard equipment but offers difficulties in the way of imperfect contacts, unsatisfactory actuating devices, and the restricted conditions under which it operates successfully, not to mention the possibilities of inaccuracy through adjustment changes. It is not portable in the strict sense of the word and must be mounted on a stable support.

The following device was developed to offset some of the disadvantages of the pendulum. It is inexpensive, positive in action, and accurate within variation limits of the cycle frequencies available.

A G. E. synchronous phonograph motor type AY 257A rated at 1800 r. p. m. is used as the timer. The gear ratio of the main shaft (*1*) to the secondary shaft (*2*) is 22½ to 1 being a dual worm gear drive (*6*). This gives a revolution of shaft (*2*) for each ¾ second. By mounting fiber studs (*4*) on this shaft to insure separation of the circuits, intervals of ¼ second can be had through the double contacts (*5*) insulated from the frame. There is room for at least two and possibly three rows of studs in this type motor. The device developed in this laboratory has two rows staggered in order to equalize the friction due to making contact. Suitable contact points of silver or alloy, depending upon the current used, can be placed on light bronze stock fashioned to shape.

Such a timer has been used in this laboratory for six months with constant running during periods of four weeks with complete success. It has been carried about on a 1500-mile trip with frequent changes in locale of operation under varying conditions without trouble. A part of the time the operation has been out of doors. The only adjustment required to date has been the tightening of tension on the clutch spring.

If the cycles approximate closely a sine wave in character the timer will run at half speed. In this case the period given is ½ second rather than ¼ second. It is not self-starting and cannot give an incorrect or false account of timing due to temporary discontinuance of the current supply. One twirl of the wheel (*6*) just prior to closing the circuit will always start the motor at full speed. Starting at half speed requires more care.

*Developed in connection with activities of the Committee on Psychology of the Highway of the National Research Council.

The motor is well built and should wear indefinitely if kept well cleaned and oiled. It is especially recommended where a semi-portable timing device is needed.

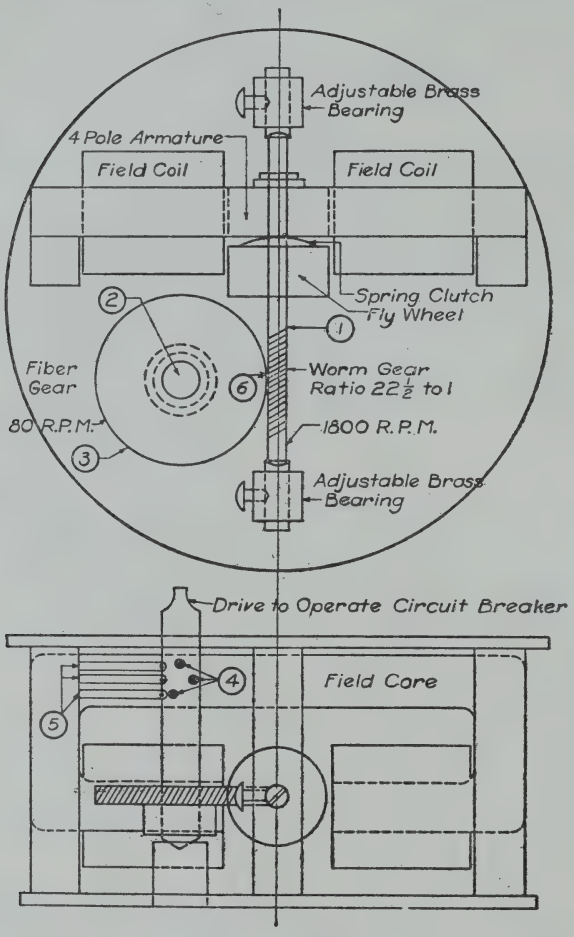

FIGURE 1

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A PRESSURE-RECORDING STYLUS

F. C. Dockeray and J. H. Taylor

In investigations in which a stylus is used it is frequently desirable to record the pressures exerted as well as the accuracy in hitting at the target. For this purpose a stylus has been devised which has proved satisfactory in the investigations which have been in progress for several months.

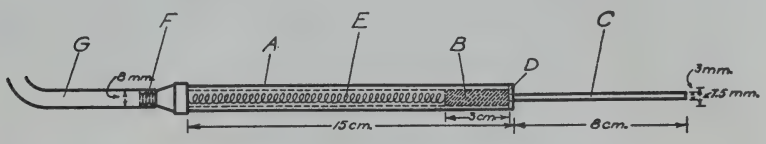

FIGURE 1

The inner wall of a brass tube (*A*), approximately 15 cm. long, was carefully honed to produce a smooth surface and uniform diameter of about 7.5 mm. A piston (*B*), 3 cm. long, with highly polished surface was fitted into this cylinder. To the piston was attached the stylus (*C*), 8 cm. long, and with a diameter of 3 mm. The collar (*D*) held the piston in place. A light coil spring (*E*) within the cylinder returns the piston after each displacement. The space between the cylinder and piston walls was sealed by the occasional insertion of a few drops of medium-grade oil.

A reduction tip (*F*) was soldered to the upper end of the cylinder. To this was attached a section of pliable rubber tubing of 4 mm. inside diameter and 8 mm. outside diameter. The other end of the rubber tubing was connected with a copper tube which leads to the recording device in the next room. This consisted of the apparatus described by Jensen (1). Tests of the constancy of pressure records over a period of several months showed that slight, medium, and maximum displacements of the stylus piston gave constant pen displacements.

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AN IMPROVEMENT OF THE HERON MAZE RECORDER

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The electrical recording system described by Heron (2) is a very useful device, making it possible to record automatically movements in a darkened problem box or maze, while avoiding the inherent disadvantage of tilting platform or treadle switches. Furthermore the electrical current passed through the contact plates is only about 18 microamperes, and apparently produces no stimulation. It is quite possible that this amount of current can be perceived, and thus it may serve as a cue in learning. Dunlap (1) states that a current of 40 microamperes is perceptible to the rat. The safest plan is to place the same potential on the contacts in both the blind alley and the true pathway in order to avoid differential cues from this source. The dotted lines in Figure 1 show the additional connections if

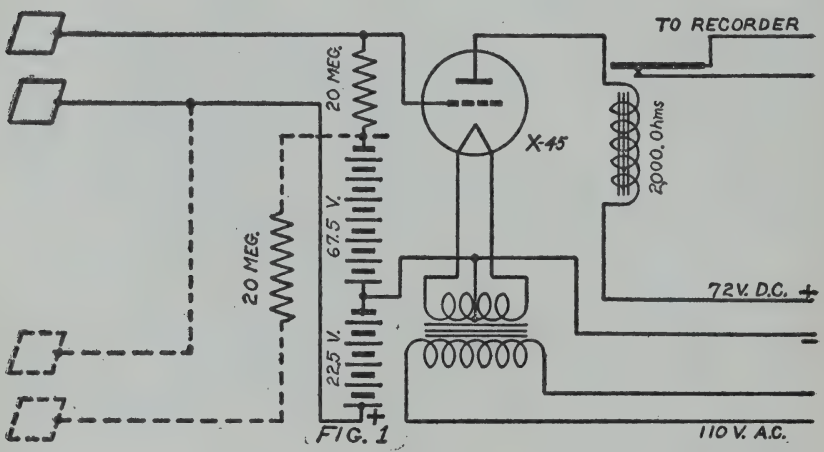

FIG. 1

only one of the alleys is connected to the recorder. In the Heron device, an electrical relay is connected in the anode circuit of a type-245 vacuum tube. The grid of this tube is normally kept at a negative potential by means of a battery connected through a 20-megohm resistor to the grid. The anode current of the tube is very low in this situation, and the relay remains open. Contact with the plates connected to the grid and filament of the tube reduces the grid potential nearly to zero. The anode current immediately increases, causing the relay to operate.

AN IMPROVED CIRCUIT

In order to operate the device successfully it is necessary to keep the anode voltage of the vacuum tube at a critical value. If it falls only

slightly below this value, the relays do not always operate; if it rises too far above this value, the relays chatter and may close without being actuated by the passage of the animal. Sometimes the action of the relays is not independent when a number of units are used.

The anode voltage adjustment has been made non-critical by using the circuit shown in the diagram. It is apparent that when contact is made the grid is given a considerable positive potential, instead of merely being reduced to a low negative value. The anode voltage necessary to operate the relays is also lower when this circuit is used. A six-unit recorder of this type has recently been operated on the 72-volt D. C. laboratory circuit, for four months without adjustment.

Insulation of the Contacts

During preliminary training of the animals in a discrimination problem the recorder frequently became inoperative due to moisture from urine which caused short circuits between the contact plates. It was often necessary to wait several hours before the trouble disappeared. Shellac and similar insulators did not remedy the situation. Impregnating the floor boards with paraffin before installing the contacts almost completely eliminated the trouble.

A Simplified Position Indicator

It is sometimes necessary to follow movements in a darkened problem box, or similar situation, although automatic recording may not be necessary. The electrical relay may be replaced by a 120-volt corona glow lamp of commercial type. This arrangement is much more economical, particularly if a large number of indicators are to be used. A direct current anode supply is not needed in this case as alternating current is just as satisfactory. A small 45-volt radio "B" battery in series with the 110-volt A. C. lighting circuit, giving a total potential of 155 volts, has been found to work well as an anode current source.

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A NEW DEVICE FOR STUDYING LEARNING

T. A. Langlie

This apparatus is composed of several pieces in series. They will be described individually and in relation to each other. The "learner" sees and works with two circular brass and bakelite boxes. (See Figure 1.) One of them (*A*) is a specially constructed switch box. In this box there are six mercury switches activated by six plungers. When a plunger is depressed, mercury is forced up in a cup, making contact with five brass electrodes. A sixth (battery contact) is immersed in the mercury at all times. Two of these switches have all five contacts made through relays, and the rest have four, three, two, and one contact made respectively. Using radio tube bases for plugs the ordering of switches and number of contacts can be altered at will.

The other circular box (*B*) contains sixteen lights—one in the center, colored red, and fifteen around the periphery. These fifteen lights are numbered from "1" to "15" in order. They are connected through relays with the mercury switch box in such a way that any two plungers will turn on one light. If the two plungers pressed is a correct combination the corresponding numbered light flashes. If it is incorrect, the red light comes on. The problem set the subject is to flash the numbered lights in order from 1 to 15 without turning on the red light. There are just fifteen combinations of two plungers each.

The rest of this apparatus is composed of an automatic typewriter, a specially constructed distributor, relays, and counters. (See Figure 2). When any two plungers are depressed by the subject, a relay is activated by one switch and a circuit is closed through the relay by the other switch. (See Figure 3.) Five relays (*C*) are used and they "make" five, four, three, two, and one contacts respectively, as well as breaking one circuit to throw the other relays out of action. Each of these relays is activated by one of the switches in the switch box and the sixth switch closes circuits which go through all of the relays. In the wiring diagram it will be seen that switches *1* to *5* activate relays, and switches *2* to *6* close circuits through the relays. When relay *1* is activated, the circuit to relays *2* to *5* is broken. Likewise, when relay *2* is activated the circuit to relays *3* to *5* is opened. In this way only one relay can be activated at one time.

The fifteen circuits closed through the relays go to solenoids in the typewriter (*D*) which depress keys, thereby recording on a tape the combination of plungers pressed by the subject. The tape is fed through the rollers of the typewriter at a constant speed by a motor and gears. A Veeder counter attached to the roller records the number of revolutions and hence the time taken by a subject can be determined. A new circuit is closed by each solenoid in the typewriter, these going through the distributor (*E*).

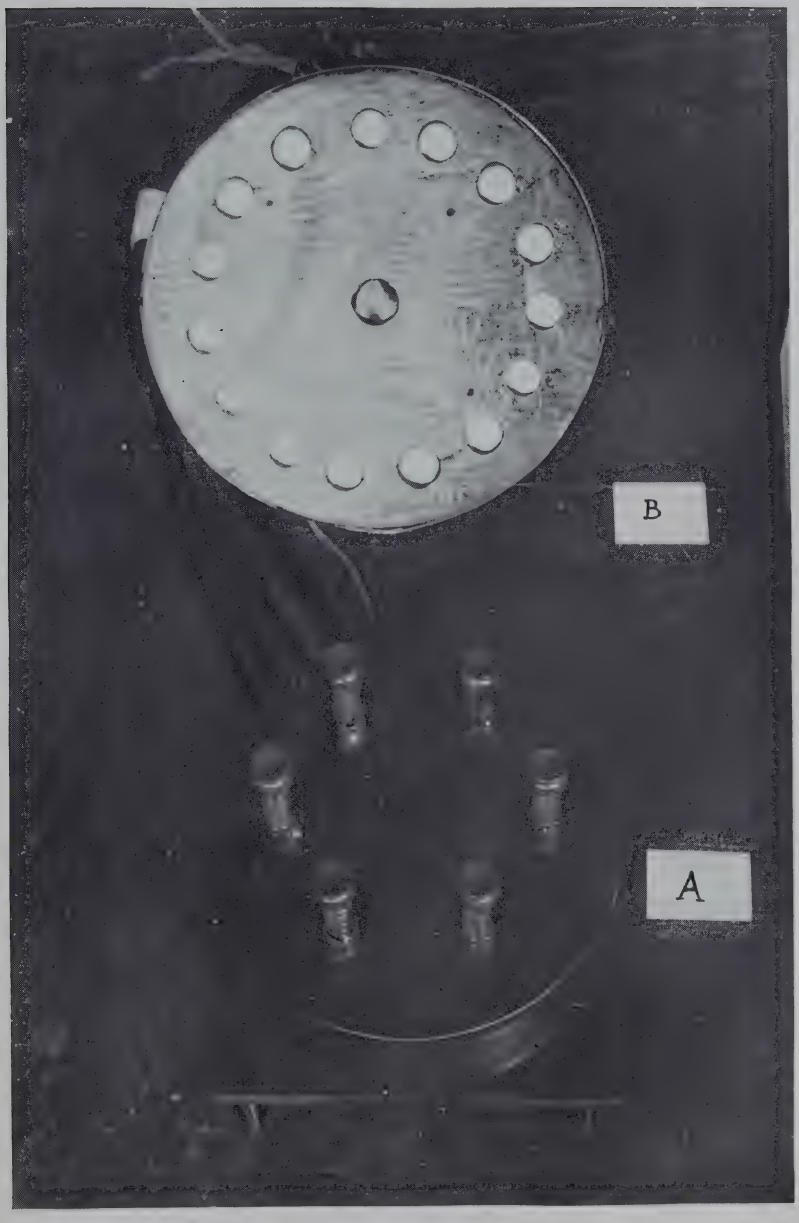

FIGURE 1

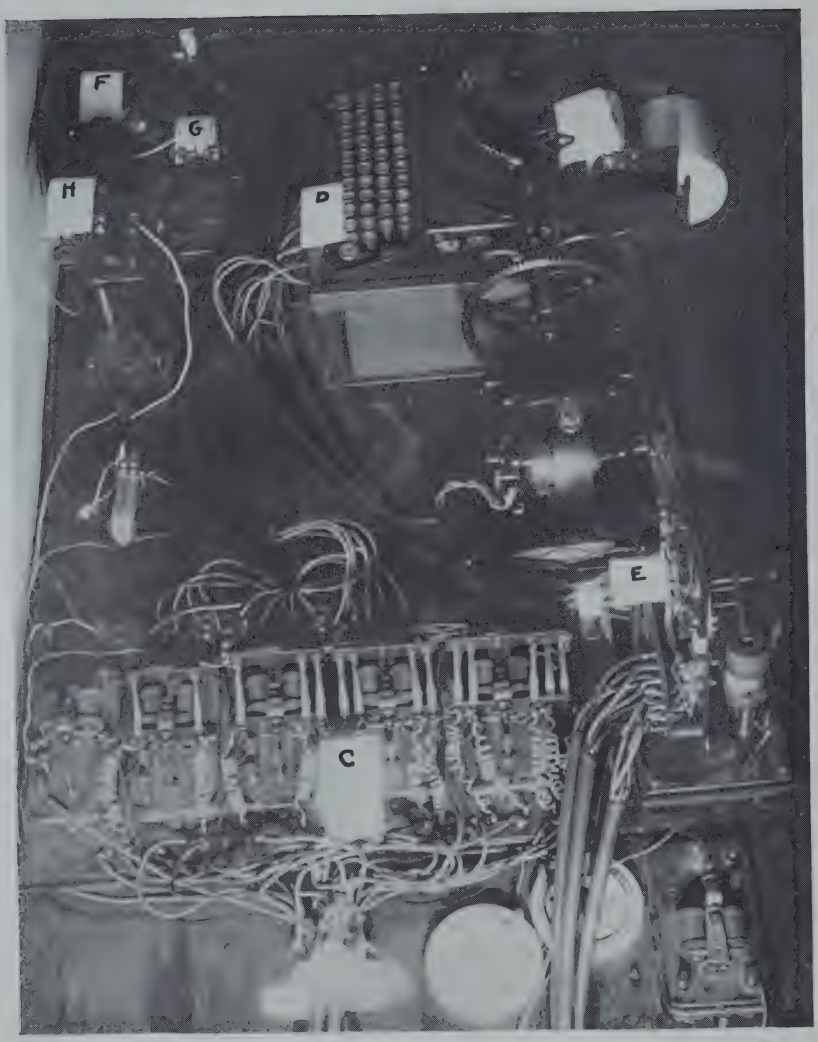

FIGURE 2

(In Figure 3 two circuits are shown. The rest follow the same path as the "red light" circuit except as each in turn is deflected to its own numbered light.)

As will be seen in the diagram, an error results in turning on the red

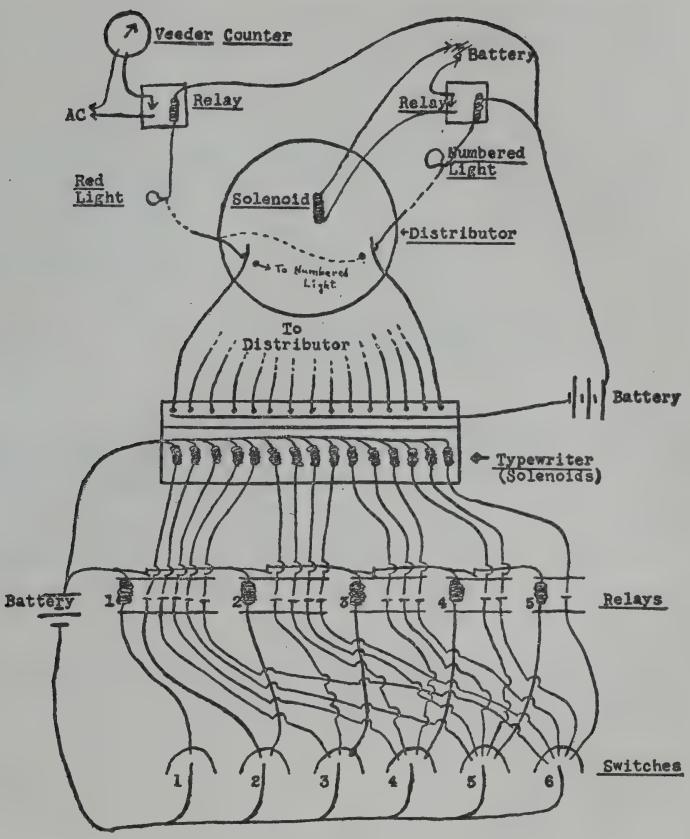

FIGURE 3
WIRING DIAGRAM

light as well as activating a relay. This relay closes a circuit in which is located an electric Veeder counter (*F*) to record the number of errors. The correct combination of switches results in closing a circuit through the distributor and thence to the light box and the particular numbered light to be associated with the combination of plungers selected. A relay in this circuit closes another circuit through a solenoid on the distributor which resets the distributor for the next numbered light.

The distributor is composed of a circle of spring contacts, each resting on a stationary contact except as a peg on a wheel pushes each in turn against another stationary contact. As a result, one circuit is closed through

one numbered light and the rest go through the red light as described above. The wheel is moved one fifteenth of its circumference each time the solenoid is activated. The adjustment occurs as the core of the solenoid comes to rest so the setting which exists as the plungers are pushed continues until the plungers are released and the first circuits are opened. A counter is attached to the axle to register number of revolutions and hence number of trials. Two additional contacts are made as the wheel revolves—one when the change from a setting for light number 1 to light number 2 takes place, the other as the wheel returns to number 1 setting from number 15. The first circuit activates a relay (*G*) which closes another circuit and maintains it until an error is made by the subject. The second circuit goes through the relay (when closed) and throws out a master switch (*H*), stopping operation of the entire set-up. This wiring is not shown in the diagram since it is not essential to the operation of the apparatus.

Power is obtained from Edison batteries for all relays and solenoids, and A. C. is used for the Veeder Electric Counter and the Constant Speed motor. A battery charger is operated while the apparatus is functioning since there is a heavy drain on the batteries.

This device can be used in a variety of ways for studying learning. At present it is being used in a study of kinds of errors that are made. The problem seems to be very similar to a maze problem with similar learning curves and errors. It has also been used to investigate the relationship between types of errors and emotional stability. It appears that this situation is analogous to situations to which Hamilton (1) exposed infra-human animals in his studies of "perseverance reactions," and we have obtained all the types of attack on this problem with college students that he described. Furthermore, one study (2), using a forerunner of this apparatus, yielded significant correlations between the percent of repetitive (or perseverative) errors and the Bernreuter Personality Inventory scored for Neurotic Trend (B1-N). Some advantages of this device are:

1. It is completely automatic, including typing of kinds of reactions made by the subject, recording number of errors, time, and trials.

2. Combinations of switches can be altered by shifting the plugs in the switch box.

3. Spatial relations of a simple sort are eliminated since the plungers are arranged hexagonally and each plunger is used five times.

4. The difficulty of the problem is great enough for college students and adults. A simpler set-up could be made by using five switches and ten lights.

5. There is evidence that this problem is for human subjects what the maze problem is for rats. Further research will support or destroy this

hypothesis, and if established, we have a means of comparing more directly the mass of data about rat learning with human learning in an analogous situation.

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A WATER ELEVATED MAZE

Siegen K. Chou

In a recent summary of all existing maze constructions (2), the present writer distinguished no less than thirty-six varieties of maze designs, while presenting at the same time the thirty-seventh technique—the so-called "Rolling-Ball Maze." However, one significant adaptation from some of the well-known forms of construction had been overlooked in the former exhaustive summary. This is the obvious combination of Miles's narrow-path elevated maze for rats (5) with McDougall-Li's water-tank method.[1] Since this innovation of a *water elevated maze* for rats is, so far as the present author is aware, new and convenient for many reasons, a few words of description and development are perhaps not out of place here.

The essential idea is to immerse Miles's skeleton (5) or Dennis block (3) elevated maze in water so that the surface of the elevated path is just a trifle above water level. In this technique, the rat is required to traverse the dry elevated path, to swim from water to elevated path, or to dive from elevated path to the water and then to climb up the dry path again. These variations of procedure depend merely upon the purpose of the experiment and hence numerous animal behavior problems are suggested by this little innovation of the two already well-known techniques when they are deliberately combined into one technique. Some of the obvious behavioral problems of the rat on this type of *water elevated maze* may be mentioned simply as hints for further research.

1. If a rat is already accustomed to run on the elevated and to swim in the water maze separately, what behavioral changes would it manifest when it is put in a combined *water elevated maze?* Would it both run on the elevated path and swim between the elevations? Or would it tend to run more than to swim? What is the effect of substituting a water gap as a blind alley? Would the rat tend to jump over, or to swim over (if

[1]The origin of the water maze is mentioned in (4, p. 295).

the distance is beyond the optimal distance of jumping)? Can water be utilized as an obstruction to the rat?

2. If the rat is first trained to run on the elevated and then to swim in the water, and *vice versa,* would the general goal-seeking tendency be strong enough to dispose it to choose running more than to endure swimming (assuming that swimming is more annoying to the rat)? In other words, what is the relative merit of dry path versus water tank in inducing food-seeking activities of the rat?

3. Would the reliability of the elevated skeleton or block maze be increased if it is immersed in water just up to the surface of the elevated path such that the rat may either deliberately choose to leave the path and swim or to take care not to get wet? It is a fact that the rat in water seeks for land. On the other hand, can it be taught to seek for water while on land? If so, what are some of the conditions for this possibility?

The last series of questions suggest a possible remedy for the elevated skeleton or block maze where the rat tends to explore geotropically at the early stage of training. When the writer first introduced and duplicated Miles's elevated skeleton maze in 1931 in the Tsing Hua Laboratory, he and others noted the fact that the rat insists on exploring down the elevated path especially during the preliminary training period. Although this geotropic exploring behavior is itself a significant problem that deserves experimental investigation, the advantages of the elevated over alley maze seem to be much lessened by this unfortunate weakness. Indeed the ground-seeking tendency of the rat is so strong that not infrequently the animal slid down the side posts of the skeleton with the result that its feet were hurt, thus causing much bleeding which led to much uneasiness on the part of the experimenter. It is curious that neither Miles nor Dennis mentioned this *ground-seeking tendency* of the rat on the elevated maze.

However, if the combined *water elevated maze* is used, naturally the ground under water will not be sought by the rat unless it loves open space more than it avoids water. Thus the water level serves as an immaterial, so to speak, alley to the elevated maze, eliminating, on the one hand, all the objections inherent in the alley maze and, on the other, the new objection of ground exploration while on the elevated narrow path.

Various adaptations of this dry-path-in-water method are possible. A mere platform surrounded with water may serve as an island on which the rat may be put to see its reaction toward escaping from solitary confinement. This island platform may have several exits to surrounding open spaces or may have no connecting paths at all with a surrounding open space so that rats may learn actual crossing of bridges or jumping over an alley of water. Platforms of various sizes may be constructed with common final paths leading to mainland in such a way that a somewhat analogous multiple-choice problem may be set to the rat. Assuming that

the rat tends to seek for larger platforms, quantitative determinations of the frequency of entrance to the various sizes of platforms may be determined. Choice of relative sizes of the platforms, not of the absolute, may be trained in the rat just as perhaps in the visual response to the relation of "larger-than" or "smaller-than" configurations. Other problems of course suggest themselves when the general significance of the combined *water elevated maze* is once understood.

The idea of combining the water-tank procedure with the elevated narrow-path technique occurred to the writer in the early part of 1931 when he was seeking for a new technique to investigate social behavior, especially rivalry or competition, in the white rat. The project on social behavior of the white rat was first mentioned in a local university weekly publication which appeared June 1, 1931.[2] Mr. Yung C. Mou, at the suggestion of the writer, used Miles's elevated narrow-path maze in a quantitative study of the rôle of kinaesthesis versus vision in maze learning. This was in the first part of 1932. At the beginning of the academic year 1932-1933, a graduate student, Mr. J. T. Lei, was interested in the social behavior of the white rat, and to him the first problem of rivalry or competition, together with some detailed hints of the construction of elevated paths for two or more rats running together, was suggested. Mr. Lei is now working on this problem. The *water elevated maze* has now also been adopted by another graduate student, Mr. N. C. Shen.

The present writer suggests that a combined *water elevated maze* may be used for investigating various new significant behavior problems of the white rat or similar laboratory animals. This elevated-path-in-water idea is, I think, significantly new and deserves experimental trial. It suggests several new problems for research: (1) the relative merit of the elevated maze in the open or immersed in water may be experimentally compared; (2) running on dry path and swimming in water is perhaps characteristically different; and (3) the reliability of the elevated skeleton or block maze is perhaps appreciably increased by immersing it in water so that the animal will have no chance to explore downwards. The last problem is certainly a justification for trying out the new technique seriously, because the author noted early in 1932 that rats newly put on the narrow elevated skeleton path of Miles's design tend to explore downwards persistently. Although this stronger downward-exploring tendency at the early stage of practice is a significant problem in and by itself, it is a distinct objection to the Miles's elevated skeleton maze technique, where the forward-going tendency is theoretically better controlled at first sight than in the traditional alley maze. Experiments are now in progress in the Tsing Hua

[2]The problem of rivalry in the white rat was first put in print in (5, p. 115) from among a list of eleven experimental problems suggested for research and has since been taken up by J. T. Lei.

Laboratory to investigate the significance of this downward-exploring tendency in comparison with the forward-going tendency on the Miles elevated maze with McDougall-Li's method of water-tank control.

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BOOKS

L. L. THURSTONE. *The Theory of Multiple Factors.* Chicago: Author, 1933. Pp. vii+65.

Putting aside for the moment the question of what a factor may be, we may safely say that the theory of its logical properties and interactions has now pretty certainly achieved maturity. There was always something a trifle unreal about Spearman's two-factor theory (with four factors), if for nothing but that it seemed to be forever coming unbuttoned and having to be smuggled hastily away to have a new set of provisos sewed on before anybody began to snicker; and the gorgeous polemics with which its protagonist was wont to entertain us somehow smacked of the romantic days when stags were brought to bay by the lowly pack, etc. E. B. Wilson's extraordinary reviews were the first hint that the old era was passing; then came Kelley's *Crossroads,* horribly unwieldy but evincing an entirely justified conviction that if the business was to go on it would have to be generalized; then a seepage of writings, culminating in one by, so to speak, the chief of engineers himself, Garnett, the purport of all of which was unanimously that g was not unique; and finally Thurstone, in whose present brochure it seems clear that the two-factor theory is relegated to the same place in the history of statistics that Watling Island occupies in that of America. It is, that is to say, now definitely a special case, whose only importance is that it was encountered first and pointed the way for investigators mathematically more powerfully equipped.

To return for a moment to the question of what it all means anyway: As long as Spearman was certain that g was something more than "the initial of ghost," it was possible to make not too unreasonable guesses in the way of relating it to something fairly unslippery. It might, e.g., be something like speed of nervous conduction, facility of formation of synaptic connections, etc.; Spearman himself suggested nervous energy, a bit less tangible but still within the limits of the comprehensible; H. F. Adams raised the disturbing suggestion that it might be the influence of the thing aimed at, as the shot patterns of different marksmen would have a certain commonness of position ascribable to the unity of the target—which is even more comprehensible, but of next to no practical significance. But this pastime has evaporated with the new developments. It is now frankly a matter of determining the smallest number of uncorrelated common factors which will *account for* a given table of intercorrelations—precisely as if the general superintendent of a railroad were faced with the problem of determining the minimum personnel necessary to move a given

tonnage in a given time under certain geographical and meteorological conditions. And this puts a different face on the matter, does it not? By way of consolation we are told, to be sure, that all science now proceeds in terms of constructs which are more or less frankly out of touch with reality—or even that it is no longer *á la mode* to be concerned with reality at all. But this is somehow pretty cold comfort. If we are to make no attempt to elaborate stable units which have at least some reasonably constant relation to the gross observable facts of structure and function, what is it all about? Four general factors may be postulated for the intercorrelation table we have before us today, drawn from seven intelligence tests on 500 Jewish seven-year-olds in New York; the same seven tests applied to 300 Swedes in Minneapolis tomorrow may need five, or three, or four—and Statisticus himself can't know to what extent, if any, they are the same four (if sameness has any meaning in this context) or what connection, if any, they have with the general body of knowledge about Homo sap. It is lovely logic; but so, probably, is bridge.

Thurstone is, as always, open-eyed and lucid on these points. A few excerpts from his charmingly written Introduction will serve by way of conclusion, both to give his viewpoint on these considerations and to indicate the scope of the brochure:

> The trouble is that the debaters talk as though the factors were some sort of reality, that they should be searched for and tagged when found; but this is the same troublesome illusion that appears elsewhere in science. . . .
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> But the force is also such a fiction. Nobody has ever seen or touched a force . . . . the only criterion here as elsewhere in science is that we shall have peace of mind about the phenomena that we are studying. If our schemes are inconsistent or if the phenomena are inconsistent with our schemes, then in order to regain peace of mind we must either reinterpret the phenomena according to our scheme or change the scheme to fit our experiences. But we try to save our scheme first. To ask whether there "really" are several forces when one is sufficient, or to ask whether there "really" are several factors when one is sufficient, is foolish because it is in the last analysis only a matter of preference and convenience anyway. . . . This point of view will save the student much useless labor.
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> The "probable error phobia" is a mental disease which afflicts psychologists particularly. . . . The patients refuse to write down interesting numbers just because they don't know how to compute the probable errors. They even refuse to think about numbers without the entanglement of probable errors, and consequently their style is much cramped. . . . The student should be warned that it is really not a crime to give a correlation coefficient without a probable error. . . .
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> The first section is a description of the problem and the development of concepts that are useful in dealing with the prob-

lem. The second section is a generalized least square solution which is rather illuminating but exceedingly laborious in application. The fundamental factor theorem is there stated, namely, that *the number of factors required to account for a table of intercorrelations is the rank of their matrix.* The third section describes a least square solution by successive approximation that is applicable to any number of variables, to any number of factors, and to any required degree of accuracy. . . .

Tetrad differences need no longer be computed. Instead, we can get the first factor loadings or coordinates of the tests, and these constitute the correlations between the tests and the most conspicuous common factor, which may be called "g" if the variables are tests of intelligence. . . .

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L. L. THURSTONE. *A Simplified Multiple Factor Method and an Outline of the Computations.* Chicago, Author, 1933. Pp. 25.

A supplement to the recent monograph *The Theory of Multiple Factors;* in the former work the structure of the problem and the terminology were mapped out, and in the present one the principal modification is the elimination of sub-groups and the substitution of a "center of gravity method" (in hyperspace) for the former least square method. The saving is the difference between five hours and "several weeks" for the nine-variable problem used. The concept of the observer at the origin looking out through hyperspace to determine where the clusterings occur, in order that he may put the next axes through them, is introduced by way of clarification, together with the simplifying idea of the mirror image of a test obtained by changing its sign. There is an outline of the calculations which is amenable to use by any competent computer; full tables are appended by way of illustration.

There is about as much to be said in evaluation of this now apparently complete generalization of an intriguing problem as about a particularly skilful bit of major surgery; that is—after one has emitted a sufficiency of lyrics—nothing. Like a Rembrandt or a June day, there it is. No doubt the connoisseurs will delight to adorn it with a bit of filigree here and a *je ne sais quoi* over yonder; but essentially the job is done, and that's that.

Come to think of it, there is one thing, though; what does one do with it? This question is not raised with any intention of being facetious; it springs from a feeling of real perplexity. Spearman originally thought his findings pointed to a definite entity, which was great news; but with the gradual seeping in of one group-factor after another we seem to have come regretfully to the realization that the factor techniques are after all

only methods of "as-if" description, whose ultimate usefulness is probably to enable a compact formulation of the interrelations of a set of coefficients. And so—with a perfected instrument, just where are we? To what realities of the mind do—or could—these wholly empirical factors to which the instrument will mechanically boil down any set of coefficients, and whose stability from set to set is entirely problematical, correspond? We await hopefully a thoroughgoing discussion of the matter, preferably by somebody with Thurstone's acumen (if there be any such) but without his "realist," or better impossibilist, philosophy.

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